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VOL. IV.

Illustrated with Engravings:

BY WILLIAM NICHOLSON.

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BRITISH MUSEUM NATURAL HISTORY.

PREFACE.

ON the Completion of this fourth Volume I have the Pleasure to give the Names of the Authors and List of Engravings.

The Authors of original Papers are John Gough, Esq.; N. N.; J. Fletcher, Esq.; Ez. Walker, Esq.; J. Harriott, Esq.; Mr. W. Wilson; Dr. Priestley, F. R. S. &c.; Thomas Young, M. D. F. R. S. &c.; Mr. Benj. Hooke; W. N.; John Bostock, M. D.; G. C.; R. B.; The Rev. Wm. Gregor; Mr. Wm. Henry; Z.; Π; and Lawson Huddleston, Esq.—Of foreign Works, G. Dalarive; Rochon; Guiton; Cadet; Gall; Bojames; Klaproth; Jumilhac; Saint Victor; J. B. Berard; L. Reynier; Proust; and Fourcroy.—And of English Memoirs abridged or extracted, R. Chenevix, Esq. F. R. S.; H. Davy, Esq.; S. Tennant, Esq. F. R. S.; Mr. J. Dalton; W. H. Wollaston, M. D. F. R. S.; W. Herschel, L. L. D. F. R. S.; R. Kirwan, L. L. D. F. R. S.; and M. des Lozieres.

Of the Engravings the Subjects are, 1. A new sliding Stop for Air-Pumps and other Uses. 2. A simple and secure Metallic Joint for Tubes. 3. Mr. Harriott's Syphon Engine for raising and lowering Weights. 4. New Method of measuring the Action of Bodies in refracting Light, whether they be transparent or opake; by Dr. Wollaston. 5. Mr. Hooke's Blow Pipe by Alcohol. 6. The Harmonic Sliders of Dr. Young. 7. A strong and flexible Joint for Tubes.

8. Dr.

8. Dr. Wollaston's Figures to explain the oblique Refraction of Iceland Crystal. 9. Mr. Dalton's Sketches of the Rivers of England and Wales, and the Districts which supply them with Water. 10. Figure to illustrate Mr. Gough's Theory of sonorous Undulation. 11. The Craniognomic System of Dr. Gall. 12. An Instrument for extracting hard Bodies out of the Œsophagus. 13. A new Reflecting Quadrant by Mr. Ez. Walker. 14. Mr. Huddlestone's Method of floating Boats from one Level to another, without Lofs of Water. 15. Engine for blowing up the Stumps of Trees, by Cit. Saint Victor; and 16. Two Drawings of a Magazine Pistol, which discharges Nine Balls in Succession, by once loading.

Soho Square, April 28, 1803.

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JANUARY, 1803.

ARTICLE I.

On the Nature of the Grave Harmonics. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

OUR fenfe of time arifes, as Mr. Locke juftly obferves, from the conftant fucceffion of ideas in the mind; or to fpeak perhaps in more intelligible language, it is the refult of the attention being occupied by an uninterrupted train of changeable perceptions. Time is on this account, capable of increafe and diminution; it is therefore a fpecies of the abftract term, Magnitude; in confequence of which, the parts of it poffefs all the properties contained in the abftract term, Ratio, and are proper objects of the doctrine of proportion. A great variety of proportion takes place amongft the conftituent parts of compound beings of the fame denomination, and gives birth to a clafs of phenomena in the philofophy of the human underftanding; which can only be explained by an hypothefis afcribing to man a faculty that compares thefe ratios, and perceives the effect produced by them. Of this defcription are the differences perceived by the fenfe of feeling in the texture of bodies,

Time is a fpecies of magnitude; and its parts have the habitude of ratio to each other.

The mind compares ratios together as well as their parts.

Phyfical effects thus difcriminated.

dies, the difcrimination of perfons, which is the office of the eye, and thofe modifications of time, called fymphony and harmony, which are judged of by the ear.

The ear affords the conception of a permanent found, only when the vibrations are too frequent to be feparately confidered;

Each fenfe reckons time, by the train of its own ideas alone; the auditory organ on this account meafures the lapfe of it by found. Now if the ear had the power of confidering the fmalleft intervals of time apart, it would perceive the conftituent vibrations of founds as feparate diftinct things, attracting its notice fucceffively; in which cafe, the human mind could not poffibly form a conception of a permanent found: but it is a maxim in harmonics, that a certain number of vibrations in a fecond is required to give a fenfible degree of duration to the note of a ftring; I fhall at prefent make ufe of 12 for this number. The preceding fact afcertains an effential point of my theory, for it fixes the leaft interval of time that the ear can

but the ratios of frequency are perceived, and excite ideas of acute and grave.

contemplate apart; this organ therefore takes in the grofs all magnitudes of this defcription, which are too fmall to be examined in detail: in this cafe it acquires the ideas of acute and grave, by comparing two or more trains of vibrations; the intervals of which are equal in each feries taken feparately, but greater and lefs in the whole number taken together.

Two continuous founds, heard together, have their vibrations arranged in cycles of equal duration perceptible by the ear: If the duration be long enough, each cycle is diftinguifhed;

When two founds, thus conftituted, are heard in concert, the vibrations producing them are arranged in cycles, no one of which continues for a longer or fhorter time than the reft; and their effect is perceived by the ear, which becomes fenfible of their prefence. For when each cycle of a feries, feparately confidered, exceeds the twelfth part of a fecond, the fenfe of hearing recognizes each point of divifion made by the coincidence of the vibrations which feparate the contiguous cycles: this circumftance enables the fenfe to contemplate

if not, the points of recurrence will have the effect of fimple periods,

thefe periods apart, and to comprehend their origin. On the contrary, when the duration of a cycle belonging to a compound feries does not exceed the twelfth part of a fecond, the interval proves too fmall to be meafured by the ear; it therefore efcapes notice in a feparate ftate; for the points of divifion recur too frequently to be obferved. When the auditory organ finds itfelf in circumftances anfwering to the preceding defcription, it has but one method to purfue; which is to treat thefe derivative intervals in the fame manner it treats all pe-

and afford a mufical found, or grave harmonic,

riods which are fingly too fmall for its comprehenfion: it therefore reduces them to a fimple mufical found, correfponding

ing in pitch to a ſtring; which vibrates once in the time of each ſucceſſive cycle: A grave harmonic is on this account always a lower note than any of its conſtituents, ſeeing the time of a cycle exceeds the greateſt vibration that enters into the compoſition of it. The ſtrength of a grave harmonic is alſo weak, when compared with the notes compoſing it, becauſe theſe ſecondary ſounds, being nothing more than certain unavoidable efforts of the imagination, they aſſume the character of a feeble ſound, which is juſt ſtrong enough to be heard in the company of one or more louder tones; for the power of the imagination is always inferior to external impreſſions, except in fits of inſanity, when the organs of ſenſe appear to be blunted by phyſical cauſes. This inferiority of the fancy may be aſſigned as the reaſon of imaginary ſounds, which it frequently creates, being conſtantly faint and apparently diſtant.

lower than either of the two original ſounds, and fainter. It is the reſult of imagination.

Dr. Young mentions a trait of the grave harmonics, which ſpares my own authority, and gives an opportunity to quote his for the following fact, which is of the firſt moment to the preſent theory: The grave harmonics always keep the direction of the ears, let the poſition of the head be changed as oft as you pleaſe, reſembling in this circumſtance a ſhrill piping note, called the ringing of the ears; which every one aſcribes to a ſlight affection of the auditory duct, becauſe it differs from external ſounds in having no fixed direction. The grave harmonics agree with the ringing of the ears in this remarkable particular; which is a ſtrong proof that their immediate cauſe is ſeated in the perſon of the hearer; and it is evident from the nature of things, that this cauſe originates in the mind, ſeeing that the organ does not labour at the time under a phyſical impediment.

In proof of which, the fact *that it has no direction* is urged.

I have now given a general theory of theſe ideal ſounds, in compliance with Dr. Young's injunctions, but am not able to perceive any connection between the ſubject and the queſtion in debate. This was the reaſon why I diſregarded in my laſt, one of the Doctor's remarks, which he has ſince called a challenge, perhaps in a petulant manner: a little more argument and a little leſs ill humour would certainly promote both his cauſe and reputation. The Doctor endeavours to perſuade me, that I miſunderſtand his idea of coaleſcence. My idea of the ſubject is ſimply this, that ſounds cannot coaleſce; conſequently that all compounds are nothing but a number of diſ-

Concluding remarks.

tinct cotemporary founds: if the Doctor will condefcend to fay, he contends for nothing more, the difpute is at an end; but fhould he reject this propofal, I advife him to attempt the refutation of my paper by fober arguments, and to tranfmit his thoughts to that Society which publifhed mine; where, without doubt, they will meet with a candid reception.

JOHN GOUGH.

Middlefhaw, near Kendal,
December 13, 1802.

II.

Bagatelles relating to the Pneumatic Apparatus. Sliding Stopcock; fimple and fecure metallic Joint for Tubes; Improvement of Read's Apparatus; original Invention of Woulfe's Bottles and Tubes; and of a Method of clofing Veffels. By N. N.

Invention of fliding ftop-cocks.

§ 1. THE prefent bagatelle writer has been frequently at a lofs, in his pneumatic experiments, to meet with ftop-cocks both cheap and perfectly air-tight, or to have them eafily repaired when out of order. In a converfation he had on this fubject with Dr. Fifcher, *ci-devant* Aftronomer at Manheim, he underftood that Mr. Helfenfrieder, while Profeffor of Natural Philofophy at Ingoldftadt, to remedy fimilar defects in his air-pumps, ufed fteel parallelopipedons with a vertical and horizontal opening joined at right angles, and fliding between brafs-work. This furnifhed the writer with the idea of conftructing fliding ftop-cocks for a gazometer; he was conftructing fimilar inftruments. He lays no claim to the invention; but has found them anfwer extremely well.

Defcription of the cock.

Fig. 1. Pl. I. exhibits a fection of the fliding ftop-cock put together.

Fig. 2. One of the brafs parallelopipedons (*a a*, Fig. 1) againft which the pieces *b* and *c*, Fig. 1, are preffed and united by means of four fcrews.

Fig. 3. The flider by which, when brought home on one fide, the ftop-cock is opened, and, on the contrary, fhut when moved flufh on the oppofite extremity.

N. B. The bore of the whole is three-eighths of an inch diameter throughout, which is of the greateft advantage, when any

any gas is immediately transferred from the difengaging veffel to the gazometer; for if the apparatus be fufficiently air-tight as it ought to be, and the gas rapidly difengaged, an explofion may be occafioned for want of a fufficient paffage for the gas.

Fig. 4. The piece *c*, Fig. 1. Its companion *b* ends in a male fcrew to be united to the gazometer.

Fig. 5. A perfpective view of the ftop-cock, the above enumerated parts being put together.

Fig. 6. A nut provided with a female fcrew foldered to the gazometer, by means of which the ftop-cock may be applied or removed as ufual.

Fig. 7. reprefents two brafs cylinders. One end *f* is fitted by grinding to the orifice of the ftop-cock; and to the other ends *g* and *h*, between *x*, is faftened a flexible tube for experiments with the blow-pipe.

Cheap and very ufeful flexible tube for the gafes.

This flexible tube was made by twifting a brafs-wire fpirally round a long thin cylinder, covering this with oiled filk twice wrapped round, and faftened by means of thread between the grooves of the wire. It was then again varnifhed and covered in a fpiral manner with fheep-gut flit longitudinally, and again fecured with thread. Laftly, to protect the whole from external injury, it was covered with leather in the fame manner as the tubes of the inhalers. Thefe flexible tubes anfwer the fame purpofe as the very coftly ones made of elaftic gum, fimilar to the hollow bougies made for furgeons.

Fig. 8. A (broken) blowpipe, put in this place merely to fhew how its end *k* is connected with the gazometer, by fitting into the end *(f)* of the brafs end of the flexible tube.

Defcription of a joint for metallic tubes.

§ 2. In pneumatic experiments for transferring any gas, for inftance oxigen, from the air-holder to another veffel, it is frequently neceffary to have tubes which can be joined in certain directions and inclinations; and for this purpofe a contrivance has been adopted, which is well known, and is reprefented Fig. 1. Plate II. where *a* and *b* are fpherical fegments, and *c* their junction by means of a fcrew. But as this apparatus, befides that its parts are with difficulty well ground together, is liable to become imperfect, more efpecially by ftrong preffure, or a blow received on the edges of the fegments, or a grain of dufty fand interpofed between the fmall flat furfaces, the writer of thefe trifles has contrived another, delineated

A better conftruction.

delineated Fig. 2. A B C are the parts correſponding to thoſe of the preceding. It ſerves as a connecting piece, to which tubes at D and H may be joined in the requiſite directions. Its conſtruction and advantages are ſufficiently obvious from the mere inſpection of the drawing, ſo that nothing is requiſite to be added on this account. Several friends had made it after this model, and highly recommend its uſe: and ſince the making of it the writer underſtands that the ingenious Mr. Webſter, whoſe merits were never properly eſtimated by the great improver of culinary utenſils, had the ſame thought, and executed it about two years ago; he, therefore, does not claim the merit of its firſt invention, but only (perhaps) that of publiſhing it.

The ſliding ſtop-cocks, § 1, as well as theſe connecting pieces, § 2, have been executed in a maſterly manner by Mr. Hooke, optician and mathematical inſtrument maker, No. 159, Fleet-ſtreet.

Various improvements in Read's funnel apparatus.

§ 3. In Mr. Read's very ſimple and cheap pneumatic apparatus (ſee Nicholſon's Journal, new ſeries, Vol. III. page 55) two improvements have ſince been made. The firſt, that the top of the exterior tube is not ſoldered to the funnel I (Plate IV. *cit.*) but has a top ſcrewed on with a collar of leathers, and the upper part of the innermoſt tube A F projects above it $1\frac{1}{2}$ inch, and receives the braſs cylindrical end of the funnel, fitted to it by grinding. The ſecond improvement has been made by Mr. Hooke, who inſtead of ſoldering all the parts together, has joined the interior parts to the outer tube A A, in the middle about E. By theſe means, if any impurities that might impede the action of the inſtrument, ſhould happen to ſettle at the bottom C or D, or at L, the whole may be eaſily taken aſunder and cleaned.

Woulfe's apparatus was invented by Glauber.

§ 4. The whole chemical world ſpeaks, hears, and writes of Woulfe's pneumatic apparatus. The tribute of merit muſt not be denied to Mr. Woulfe; but perhaps it may be curious to know, that the original invention of this contrivance, in the ſtrict ſenſe, belongs to good honeſt father John Rudolph Glauber. If any gentleman doubt of this fact, let him conſult Glauber's works, tranſlated into Engliſh by Chriſtopher Packe, and printed, Lond. 1689, in folio, for the tranſlator, by Thomas Milbourn. The very firſt plate to Glauber's Treatiſe on Philoſophical Furnaces ſhews it, Fig. 3.

§ 5. An-

§ 5. Another bagatelle of this kind, relating to pneumatic instruments, is a pretended new method of closing bottles and other vessels air-tight *without luting or grinding*, which, a few years past, I saw recommended (I do not recollect in which of our Journals), and which consists in having a groove round the neck into which a cap fits, see Fig. 3; so that this groove *(a)* may be charged with water or mercury as circumstances require. This likewise belongs originally to Glauber, as may be seen in the plate just before quoted of the translation of his works.

Another old invention.

There is *nothing new under the sun*, says Solomon, *Eccles.* I. v. 10. and more of this kind of *old news* shall be occasionally given by,

SIR,

Your humble servant,

N. N.

December 12, 1802.

III.

Analysis of Corundum, and of some of the Substances which accompany it; with Observations on the Affinities which the Earths have been supposed to have for each other in the humid Way. By RICHARD CHENEVIX, *Esq. F. R. S. and M. R. I. A. From the Philosophical Transactions for* 1802.

§ I.

SOME kinds of corundum, such as the adamantine spar of China, and the sapphire, have already been analyzed by Mr. Klaproth. This would have rendered any further experiments unnecessary, were it not, that I have had at my disposal many kinds of corundum he did not possess, and also some substances accompanying it, which were unknown before the preceding communication of the Count de Bournon.

Kinds of corundum not examined by Klaproth.

As, from the result of my analyses, it appears that all the different kinds of corundum are nearly similar in their constituent parts, and differ only in their proportions, it would be tedious to mention every experiment I made upon each kind. I shall therefore confine myself to stating, once for all, such modes

All the kinds are similarly constituted.

modes of analyfis as were employed with ftones of a fimilar nature; and then prefent a fummary of the refults: laftly, I fhall conclude with an enquiry into a much contefted point, which lately threatened a revolution in docimaftic chemiftry.

Principal character: Extreme hardnefs.

A principal character of corundum in general, as may be found in the Count de Bournon's mineralogical defcription, is extreme hardnefs; and thence the difficulty of reducing that fubftance into fine powder will be eafily conceived. We are told by docimaftic chemifts, that the moft advantageous method of pulverizing hard ftones, is to make them red-hot; and, in that ftate, to plunge them into cold water. But I found that this operation, when performed but once, was by no means fufficient for corundum. I therefore repeated it, till the ftone appeared to be fiffured in every direction. After this, the fpecimen to be pulverized was put into a fteel mortar, about three-fourths of an inch in diameter, and three inches in depth, into which a fteel peftle was very clofely adjufted. A few blows upon the peftle caufed the ftone to crumble; and the fragments were then eafily reduced into an impalpable powder, in an agate mortar, with a peftle of the fame material. The abrafion from the mortar, ufual in the pulverization of hard ftones, was much diminifhed by the above precaution; rubies and fapphires being, in a fhort time, ground to a powder nearly as minute as the fineft precipitate.

Pulverized after repeated ignition and quenching.

Efficacy of the procefs.

The great difficulty of fufing corundum with alkali, renders that procefs objectionable.

M. Klaproth in his analyfis before-mentioned, had obferved with how much difficulty the ftones were acted upon by potafh or foda. I found that the greateft heat a filver crucible could fupport, without melting, was not fufficient to produce a fatisfactory fufion of one part of corundum, with fix parts of either of thofe alkalis; nor did an expofure to that temperature during feveral hours, feem to render the treatment more effectual. Not more than half the quantity of the corundum was ever rendered foluble in any acid; and what remained was the powder of the ftone, wholly unchanged. The repeated filtrations and evaporations with which this treatment muft be attended, not only render it tedious, but alfo produce uncertainty in the refults. Even when very finely powdered corundum was expofed, with fix times its weight of potafh, in a platina crucible, to a heat of 140° of Wedgwood, for two hours together, it was not acted upon in fuch a manner as to

be

be fit for analyſis. From all theſe experiments I concluded, that ſome more efficacious mode of rendering corundum ſoluble in acids was to be ſought.

I boiled a great quantity of ſulphuric acid upon very finely powdered corundum in a platina crucible. But, although the acid, after a great length of time, had diſſolved a little of the ſtone, I did not find this method more ſatisfactory than the others. Nitric, muriatic, and nitro-muriatic acids, were leſs effectual than the ſulphuric. Phoſphoric acid, held in fuſion with corundum, did not diſſolve any notable portion of that ſtone, or render it ſoluble in other acids.

Neither the volatile acids nor the phoſphoric have any notable action on corundum.

I then had recourſe to ſub-borate of ſoda (borax), which I found to anſwer beyond my expectation. Two parts of that ſalt, calcined, and one of corundum, enter into fuſion, at a temperature which I judged to be about 80° of Wedgwood *; and a glaſs, more or leſs coloured, is formed. This glaſs is ſoluble in muriatic acid; and, by this method, it is eaſy to obtain a complete ſolution of corundum. My general method of operating was as follows:

Borax fuſes it well. Two parts of calc. borax and one of cor. form a glaſs ſoluble in muriatic acid.

I took one hundred grains of corundum; and, having ſeveral times made it red-hot, and plunged it into cold water, I put it into the ſteel mortar, and treated it as already mentioned. I then poured ſome very dilute muriatic acid upon it, to waſh off whatever iron might have adhered, in conſequence of its mechanical action upon the mortar. After it was dried and weighed, I put it into the agate mortar, and ground it as fine as I could. The augmentation of weight was then noted; and was always taken into account in the general reſult. I then put the whole into a platina crucible, with 200 grains of calcined ſub-borate of ſoda, and expoſed the mixture for an hour or two to a violent heat. When the crucible was cool, muriatic acid was boiled upon it and its contents; and, in about twelve hours, all the glaſs diſappeared. If I wiſhed to obtain the ſilica directly, I evaporated the whole to dryneſs; but, if otherwiſe, I precipitated by an alkaline carbonate, and waſhed the precipitate, in order to get rid of all the ſalts contained in the liquor. This latter mode I believe to be preferable. I then re-diſſolved the precipitate in muriatic acid, and evaporated for ſilica. But, as corundum contains only a ſmall portion of this earth, there

Analyſis. 100 corundum were pulverized; fuſed with 200 borax; diſſolved in muriatic acid; (evap. to dryneſs for ſilica, or) precip. by alk. carbonate; abſtract the ſalts by water; rediſſolve the precip. in mur. acid; evap. for ſilica; filter the mur. ſolution; precip. by heat with exceſs of potaſh; oxide of iron is permanently depoſited, but alumina taken up by the alk.; precip. the alumina by mur. of amm. Waſh, dry, ignite, and weigh the earths.

* I have no doubt that a lower temperature would be ſufficient.

was

was little or no appearance of jelly. When the ſilica was thus precipitated by evaporation, I filtered the liquor, and boiled it with an exceſs of potaſh. By this operation, the alumina was precipitated, and then re-diſſolved by the exceſs of potaſh, from which it was finally obtained by muriate of ammonia; the iron which had remained undiſſolved by the potaſh, having of courſe been previouſly ſeparated from the alumina. This earth, and the ſilica, after being waſhed and dried, were ignited, and thus the weight of both was obtained.

Example. Analyſis of the ſapphire.

I ſhall exemplify, in a ſingle inſtance, this mode of treatment; and then preſent the reſults obtained from the different kinds of corundum. For this purpoſe, I ſhall ſelect the blue perfect corundum, or ſapphire, as the ſtone which has been the moſt ably analyzed by Mr. Klaproth. From a view of both analyſes, the efficacy of the fuſion with borax will be evident; and the reſults of the ſeveral experiments may be compared.

100 grs. ſapphire were pulverized and then fuſed into a green glaſs with 250 borax,

1. 100 grains of ſapphire, pulverized in the agate mortar, as above ſtated, had increaſed to 105. Theſe 105 were mixed with 250 of calcined ſub-borate of ſoda, and put into a platina crucible. They were then expoſed to a violent heat for two hours, and afterwards allowed to cool. The maſs was vitrified, and had the appearance of a greeniſh blue glaſs, fiſſured in many directions.

which was diſſolved in boiling mur. acid.

2. This glaſs being ſtrongly attached to the platina crucible, the whole was put into muriatic acid, and boiled for ſome hours. By theſe means, a total and limpid ſolution was obtained.

Precip. by carb. of ammonia, was waſhed, dried, diſſolved in mur. and expoſ. to evap.

3. The matter of the ſtone was next precipitated, by ammonia not entirely ſaturated with carbonic acid; the liquor was filtered; and the precipitate well waſhed and dried. It was then rediſſolved in muriatic acid, and evaporated.

it gave a precip. of 10.25 ſilica.

4. By this evaporation a precipitate was formed, which, when well waſhed and ignited, weighed 10,25 grains, and was ſilica.

The water of waſhing and the mur. liquor were boiled with exceſs of potaſh. One grain of iron remained. Mur. of amm. was added to the alk: ſolution.

5. The liquor, together with that which had waſhed the precipitate, was boiled in a ſilver veſſel, with an exceſs of potaſh; this rediſſolved all the precipitate, except one grain.

6. Muriate of ammonia was poured into the alkaline ſolution. (No. 5.) The potaſh expelled the ammonia from the muriatic acid, and, forming muriate of potaſh, could no longer retain the earth in ſolution; a very copious precipitate, therefore, was formed.

formed. This precipitate had all the properties of alumina; and, when well washed and ignited, weighed 92 grains. Consequently, deducting 5 from the silica, for the abrasion of the mortar, we shall have for result,

The potash left united with the mur. acid, and suffered the alumina to fall. Washed and ignited it weighed 92 grs.

Comp. parts deduced.

Silica	5,25
Alumina	92
Iron	1
Loss	1,75
	100,00.

The chief difference between these proportions and those established by Mr. Klaproth, is in the silica. That chemist did not find any notable portion of it in the specimens he examined. This naturally induced me to make a very strict research into every possible means by which any silica might have been introduced into the results; whether by the borax, the alkali, or any of the other re-agents I had used. But, finding very clearly, that none of these substances did contain any, I could no longer hesitate to believe, that the proportion I have here stated, was actually contained in the sapphire I analyzed. I am likewise convinced, that no more than the quantity I have mentioned was worn from the agate mortar and pestle; for my constant practice was, to weigh them, both before and after I had used them, in scales which, when charged with four pounds on each end, turn easily with the tenth part of a grain.

As Klaproth found little silica in his analysis; this point was particularly examined.

The general results, from all the different kinds of corundum, were as follows:

Component parts of diff. kinds of corundum.

Blue perfect Corundum, or Sapphire.

Silica	5,25
Alumina	92
Iron	1
Loss	1,75
	100,00.

Red perfect Corundum, or Ruby.

Silica	7
Alumina	90
Iron	1,2
Loss	1,8
	100,0.

Imperfect Corundum from the Carnatic.

Silica	5
Alumina	91
Iron	1,5
Loss	2,5
	100,0.

Imperfect Corundum from Malabar.

Silica	7
Alumina	86,5
Iron	4
Loss	2,5
	100,0.

Imperfect

Imperfect Corundum from China.		*Imperfect Corundum from Ava.*	
Silica - - -	5,25	Silica - - -	6,5
Alumina - -	86,50	Alumina - -	87,0
Iron - - -	6,50	Iron - - -	4,5
Lofs - - -	1,75	Lofs - - -	2,0
	100,00.		100,0.

Their diff. of colour arifes from the oxidizement of the iron.

As I could not difcover chrome, or any other colouring fubftance, except iron, in thefe ftones, I can attribute their difference of colour only to the different ftate of oxidizement of the iron; but it is impoffible to afcertain what that ftate may be, from fo fmall a quantity.

The matrices of thefe ftones fubmit to the ufual analyfis.

The matrices of thefe ftones, and the fubftances accompanying them, are more eafily fufed than the fix kinds of corundum juft mentioned. The ufual and well known mode of treatment by potafh, was fufficient to render thefe fubftances foluble in the acids. Since the many experiments of Klaproth, Vauquelin, and others, the mode of analyzing mineral bodies is become fo familiar to chemifts, that I fhall mention particulars with refpect to one only of the following fubftances.

MATRIX OF CORUNDUM FROM THE PENINSULA OF INDIA.

Matrix of Indian corundum. 100 grs. fufed with potafh; diffolved in mur. acid; evaporated; diffol. in fm. excefs of acid; left 42,5 of *filica*.

1. A certain quantity of this matrix was reduced to powder, in the manner already defcribed. 100 grains of it were treated with potafh, in a filver crucible: they then afforded a limpid folution in muriatic acid. The liquor was evaporated; and, long before the mafs was entirely dry, it had affumed the appearance of a jelly. When the faline matter in the evaporating-difh was diffolved in a flight excefs of acid, a white powder remained at bottom, which had all the properties of filica, and, when wafhed and ignited, weighed 42,5 grains.

Ammonia threw down alumina and oxide.

2. Into the liquor which had ferved to wafh the above powder, I poured ammonia. A copious precipitate was thus formed, which was feparated by filtration, and well wafhed.

Carb. potafh threw down carb. lime = *lime* 15.

3. Carbonate of potafh alfo caufed a precipitate in the liquor of No. 2. This precipitate was found to be carbonate of lime, and weighed 23,5 grains, = 15 of lime.

The alumina and oxide were diff. in mur. acid; and the

4. The precipitate of No. 2. was rediffolved in muriatic acid; then boiled with an excefs of potafh, and filtered. There remained undiffolved, 3 grains, which were iron.

5. The

5. The liquor of No. 4. was precipitated by muriate of ammonia, and afforded alumina; which, being washed and ignited, weighed 37,5 grains.

alumina taken up by potash. 3 grs. *iron* remained. Mur. amm. precipitated 37,5 *alumina*. *Manganese*, a trace. Comp. parts.

I could also perceive a trace of manganese.

The proportions therefore are,

Silica - - - -	42,5
Alumina - - -	37,5
Lime - - -	15,0
Iron - - - -	3,0
Loss, with a trace of manganese -	2,0
	100,0.

By a similar treatment, the following substances, contained in this matrix, afforded the under-mentioned results.

Felspar.

Component parts of felspar;

Silica - - -	64
Alumina - - -	24
Lime - - -	6,25
Iron - - - -	2,00
Loss - - -	3,75
	100,00.

Fibrolite.

of fibrolite;

Silica - - -	38
Alumina - - -	58,25
A trace of iron, and loss -	3,75
	100,00.

This is the only stone I have ever met with, that yielded nothing but silica and alumina; for the quantity of iron was so small as hardly to be taken into account. I have repeated this analysis three times, and have not found a difference of half a grain.

Thallite in Crystals, with a rough Surface.

of rough thallite;

Silica - - -	45
Alumina - - -	28
Lime - - -	15
Iron - - - -	11
Loss - - -	1
	100.

Thallite

of prismatic thallite;

Thallite in Prisms like the Tourmalin.

Silica - - -	40
Alumina - - -	25
Lime - - -	21,5
Iron - - - -	11,5
Loss - - -	2
	100,0.

of thallite in transparent fragments;

Thallite in Fragments, of a fine transparent Yellow Colour.

Silica - - -	42
Alumina - - -	25,5
Lime - - -	16
Iron - - - -	14
Loss - - -	2,5
	100,0.

of fibrolite of the Chinese corundum;

Fibrolite accompanying the Matrix of Corundum from China.

Silica - - -	38
Alumina - - - -	46
Iron - - -	13
Loss - - - -	3
	100.

of felspar from sand of Ceylon.

Felspar from the Sand of Ceylon.

Silica - - -	68,5
Alumina - - -	20,5
Lime - - -	7
Iron - - - -	1,5
Loss - - -	2,5
	100,0.

Metallic crucibles alone must be used with earths;

As the greater part of the above substances were fusible without difficulty in potash, I preferred using a silver crucible to any other. It may be laid down as a general rule, with respect to delicate experiments, that in the treatment of metallic substances, we should not use metallic crucibles; but, in the treatment of earthy bodies, they alone are to be depended upon. The easily oxidizable metals cannot be employed; but silver and platina present advantages which no other metals seem

namely, silver and platina.

ſeem to poſſeſs. Theory would certainly give a general preference to platina, from its reſiſtance both to heat and to acids; and practice will juſtify this preference, in all but a ſingle inſtance. If a quantity of potaſh be kept for ſome time in fuſion, in a platina crucible, it will be found that the crucible has loſt ſeveral grains of its weight. The platina ſo diſſolved may be looked for in the potaſh; and, if this be ſaturated with muriatic acid, and evaporated, we ſhall find the well-known triple ſalt, formed by the combination of muriatic acid with potaſh and oxide of platina. This action of potaſh upon platina, does not depend upon any mechanical cauſe, ſuch as friction, the force that determines it being purely chemical.

Platina is preferable; except with potaſh *which acts upon it.*

If a ſalt formed by potaſh, or a ſalt formed by ammonia, be mixed with a ſalt of platina, a precipitate enſues, which is a triple ſalt; and it is by this method, that the Spaniſh government detects the platina, in the ingots of gold ſent from their American poſſeſſions. It is therefore evident, that an affinity does exiſt between potaſh and platina, in a certain ſtate; and I imagine it to be this affinity, which cauſes the oxidizement of the platina, when potaſh is kept in fuſion upon that metal.

Spaniſh method of detecting platina in ingots of gold. The ſalts of platina are precipitated (triple) by the ſalts of potaſh.

I muſt however obſerve, that my crucible was prepared by Janetty, in Paris, according to a method he has publiſhed in the *Annales de Chimie*: and that he always employs arſenic, a little of which certainly remains united to the platina. What influence arſenic may have, remains to be determined.

Is platina affected by the arſenic it contains?

Soda does not form a triple ſalt with the oxide of platina; for I have frequently kept this alkali in fuſion, in a platina crucible, for a long time; yet very little action was produced upon the metal. This fact ſeems to corroborate my aſſertion, that the affinity of potaſh for oxide of platina, determines the oxidizement of the metal.

Soda ſcarcely acts upon platina.

Whenever I ſuſpected that platina had been diſſolved, I could eaſily detect the ſmalleſt portion of it. A ſolution of platina, ſo dilute as to be nearly colourleſs, manifeſts, in a very ſhort time, the colour of a much more concentrate ſolution, and becomes reddiſh, by the addition of a ſolution of tin in muriatic acid. This I have found to be, by many degrees, the moſt ſenſible teſt for platina; and it would anſwer the purpoſes of the Spaniſh government, much better than that they uſually employ.

Muriate of tin is the moſt delicate teſt of platina.

Alkalis do not act on silver; yet the crucibles become rather more brittle by long use.

The alkalis have no immediate action upon silver; but I have observed, that crucibles of this metal, after they have been a long time in use, become somewhat more brittle than they were before.

The fixed alkalis rise by mere heat; potash more readily than soda.

Potash and soda have long been termed fixed alkalis; and it is certain that, if we compare them with ammonia, they are so. But *fixed* is an absolute term, and cannot admit of degrees. If potash, such as we obtain from Mr. Berthollet's method of preparing it, be kept in fusion at a very strong heat, it may be totally volatilized. The vapour of the alkali may be perceived in the room; and vegetable colours will undergo the change which is usually produced by alkalis. Indeed, in preparing Mr. Berthollet's potash, the vapour of the alkali may be easily perceived. Soda is not quite so volatile; though far from being fixed. It appears also, that a little water increases the volatility of both potash and soda, as happens with boracic acid. This volatility of potash, has been advantageously applied of late to the art of bleaching.

Water assists their elevation.

Potash usefully volatilized in bleaching.

§ II.

On the Affinities the Earths have been supposed to have for each other, in the humid way.

In the course of the foregoing analysis, I had occasion to make some further observations concerning a subject upon which I had been formerly engaged, namely, on the affinities the earths have been supposed to have for each other, when held in solution by acid or alkaline menstrua.

On the affinities of the earths for each other. Historical facts.

In the XXVIIIth volume of the *Annales de Chimie*, page 189, I published a paper upon the analysis of some magnesian stones. In this paper, I took notice of the following affinities of the earths for each other, namely, the affinity of alumina for magnesia, of alumina for lime, and of alumina for silica. In the XXXIst volume, page 246, there is a memoir, by Guyton de Morveau, upon a similar subject;* and he there reports some experiments of his own, by which he was induced to think, that the earths do really possess a chemical attraction for one another. Since that time, the affinity of the earths has been

Guyton's experiments.

* He has taken no notice of any of the experiments contained in my paper.

received

received among chemifts as an undoubted fact; and, at the end of Mr. Kirwan's *Effay on the Analyfis of mineral Waters*, we find a lift of earthy falts which produce a re-action upon one another, fuppofed to be caufed by an affinity that tends to unite their bafes, in the form of a precipitate, infoluble in the acids. Some other detached obfervations are to be found, in the *Journal de Phyfique*, and in the *Annales de Chimie*. The fact is certainly one of the moft important in the docimaftic art, and merits all the attention of the fkilful in that branch.

In the XLth volume of the *Annales de Chimie*, page 52, Darracq has publifhed a paper, intended as a refutation of the conclufions drawn by Guyton. I had myfelf repeated the greater part of the experiments of the latter; and the refults I obtained were exactly fimilar to thofe of Darracq. In fact, I had intended to continue the refearches; but the very fatisfactory paper of Darracq, appeared to me to render a further profecution of them totally ufelefs. However, a paragraph inferted in the *Annales de Chimie*, (Tom. XLI. p. 206.) and of which Guyton appears to be the author, fhews that he has not derived from the Memoir of Darracq, that conviction which it certainly conveys. The paragraph in queftion is founded upon a letter, written from Freyberg, by Dr. G. M. to Dr. Babington, dated December 17, 1800, and inferted in the IVth volume of Nicholfon's Journal, page 511. This letter contains an opinion which deferves to be canvaffed, as it is not perfectly juft; and the ufe Guyton has made of it, has determined me to add my obfervations to thofe of Darracq.

contefted by Darracq.

I fhall follow the order of Guyton's experiments, in the enumeration of thofe I made.

Repetition of Guyton's experiments.

Exp. 1. From a mixture of lime-water and barytes-water, Guyton obtained a precipitate. I obtained none.

Lime-water and barytes-water = precipitate.—Failed.

Exp. 2. A folution of alumina in potafh, mixed with a folution of filica in the fame, gave a precipitate, after ftanding fome time. This had been obferved by Darracq, and by Guyton, and agrees perfectly with the affinity which, before Guyton publifhed his paper, I had afferted to exift between thefe two earths.

Alumina in potafh and filica in potafh = precip.—Afcertained before.

Exp. 3, 4, 5. Lime-water, ftrontia-water, and barytes-water, produce a fomewhat fimilar effect upon a folution of filica in potafh.

Water of lime, or of ftrontia, or of barytes + to filica in potafh = precip.

 Exp.

Barytes water, and ftrontia-water = no precipitate.

Exp. 6. No precipitate took place from a mixture of barytes-water and ftrontia-water; nor from folutions of the carbonates of thofe earths, in water impregnated with carbonic acid.

Mur. lime and mur. alumina = precip.—Failed.

Exp. 7. Guyton obtained a precipitate, by mixing folutions of muriate of lime and muriate of alumina. I could not obtain any.

Mur. lime and mur. magnefia, = no precip.

Exp. 8. Solutions of muriate of lime and muriate of magnefia, when mixed, did not afford a precipitate.

Mur. lime and mur. barytes = precip.—Failed.

Exp. 9. Muriate of barytes did not, as Guyton has afferted, form a precipitate with muriate of lime. He was right in faying, that muriate of ftrontia gave no precipitate with muriate of lime.

Mur. mag. and mur. alum, = no precip.

Exp. 10. Muriate of magnefia and of alumina, afforded me no precipitate. Guyton fays, that the liquors became milky.

Mur. magnefia and mur. of barytes or ftrontia = abundant precip.—Failed.

Exp. 11. Muriate of magnefia, whether mixed with muriate of barytes or of ftrontia, afforded me no change; although Guyton fays he obtained an abundant precipitate, by mixing muriate of magnefia with muriate of barytes.

Mur. alum. and mur. barytes = precip.—Failed.

Exp. 12. Muriate of alumina and of barytes, did not, when mixed together, yield any precipitate. Guyton afferts, that there is a precipitate in this cafe.

Mur. barytes and ftrontia = no precip.

Exp. 13. Muriate of barytes and of ftrontia, did not form a precipitate. Guyton has remarked the fame.

Mur. ftrontia and alumina = no precip.

Exp. 14. From muriate of ftrontia and of alumina, I obtained no precipitate. With Guyton the liquor became milky.

Guyton was wrong in afferting that barytes has affinity for lime, magnefia and alumina; and that ftrontia attracts alumina.

From all thefe experiments it appears very clearly, that Guyton has pronounced too haftily, upon the affinity which he fuppofes barytes to entertain for lime, for magnefia, and for alumina; and that he is equally in the wrong, with regard to the affinity of ftrontia and alumina. With regard to *Exp*. 3, 4, and 5, although they appear to be true, yet it would require the refpective precipitates to be further examined, before we admit a decided affinity between the earths. The quantity of carbonic acid alfo, which muft of courfe combine with the potafh, during the treatment of the filica by that alkali, fhould be taken into account, in confidering the caufe of the precipitate.

The

The ſolutions which I uſed, of all the above ſalts, were in the moſt concentrate ſtate; therefore, in the ſtate moſt favourable for ſhowing precipitation, if any had taken place.

Cauſes of the errors in his experiments

It is not very difficult to account for the appearances that deceived Mr. Guyton in his experiments, and for the cauſe that produced them. In one inſtance, he obtained a precipitate from muriate of lime and of alumina, becauſe, in all probability, the alumina he diſſolved in muriatic acid had been precipitated from alum; and alumina, thus prepared, retains a ſmall portion of ſulphuric acid *. In the next place, it is very likely that his ſolutions were ſufficiently concentrate to give a precipitate of ſulphate of lime. The ſame was the caſe with regard to his mixture of muriate of ſtrontia with muriate of alumina. As to the general concluſion, that barytes has an affinity for lime, magneſia, and alumina, which ſtrontia does not appear to poſſeſs, it is to be explained as follows. Lime often contains a little ſulphate of lime. Mr. Guyton's magneſia, as well as his alumina, had probably been obtained from the ſulphate; and we are indebted to Mr. Berthollet, for the true nature of many ſimilar precipitates.

Barytes is a much more delicate teſt than ſtrontia, for ſulphuric acid; and therefore, barytic ſolutions were affected by quantities of ſulphuric acid, which ſtrontia could not render ſenſible. This I have aſcertained to be the caſe: for I have obtained copious precipitates, by barytes, in a liquor compoſed for the purpoſe, wherein ſtrontia did not produce the ſmalleſt cloud, or ſhow the preſence of ſulphuric acid.

He did not take due care in preparing his earths.

A little care and attention are neceſſary, in preparing the earths, which are to be diſſolved in the muriatic acid, for theſe experiments; and, if Mr. Guyton had taken the requiſite precautions, he would not have been led into error. The object to be kept in view is, to free the earth from ſulphuric acid; and, if this be obtained, there is not the ſmalleſt precipitate or cloud, in any of the caſes I have mentioned. If

* It is ſomewhat ſingular, that Guyton ſhould have obſerved this fact elſewhere. See his experiments on the diamond, in the *Annales de Chimie*. The preparation of a barytic ſalt, by alumina prepared from the ſulphate of this earth, had been obſerved by Scheele, in his *Eſſay on the Affinities of Bodies*. But that great chemiſt referred the phenomenon to its right cauſe, viz. to ſome ſulphuric acid remaining in all alumina thus prepared.

 any

any further proof be neceſſary, with regard to the cauſe of precipitates obtained in the manner ſtated by Mr. Guyton, I may add, that I have repeated his experiments, and have always found the precipitates to be ſulphate of barytes.

Mr. Kirwan has made ſimilar miſtakes.

The general concluſion to be drawn from the obſervations of Mr. Kirwan, already alluded to, is, that barytes has an affinity for lime, magneſia, and alumina, upon which earths ſtrontia does not ſeem to have any influence. But theſe miſtakes are to be accounted for in the ſame manner as thoſe of Mr. Guyton, viz. by ſulphate of barytes being much leſs ſoluble than ſulphate of ſtrontia, and therefore ſhowing the preſence of a ſmaller portion of ſulphuric acid, or, in other words, being a much more delicate teſt for that ſubſtance.

Letter in Nicholſon's Journal,

With regard to the letter already mentioned as being inſerted in Nicholſon's Journal, and which drew ſome reflections from Mr. Guyton, it is neceſſary to examine as much of it as may be thought objectionable.

aſſerting that becauſe a precipitate of ſilica is ſoluble in acids, the reſults of analyſis with alkalis muſt be fallacious.

The author ſays, that he repeated the experiments of Mr. Guyton, with an alkaline ſolution of ſilica and alumina, and that he obtained a precipitate; which precipitate, though containing ſilica, was totally ſoluble in the acids. "Here," he ſays, "the properties of the ſilex muſt be conſiderably altered. This muſt render all analyſis with alkalis ſuſpicious; and ſhews on what fallacious grounds the proud dominion of chemiſtry reſts, which ſhe has exerciſed ſo long, in ſuch an arbitrary and overbearing manner, in the mineral kingdom." This opinion is by no means likely to overthrow the pretenſions of chemiſtry; for the very circumſtance of rendering ſilica ſoluble in the acids, is one of the diſcoveries that has moſt contributed to render certain, and to extend, our knowledge of analyſis. No earthy ſubſtance is now thought fit to be ſubmitted to further experiment, till a complete ſolution of it in an acid be firſt obtained; and, when that ſolution cannot be effected directly by the acid, it is always attempted by previous fuſion with an alkali. This mode of rendering ſilica ſoluble in acids, is no new diſcovery; it has been long known; and the analyſis of minerals has never been brought ſo near to truth, as ſince it has become an indiſpenſable condition.

This fact is on the contrary the greateſt improvement in modern analyſis.

Alumina attracts ſilica;

I have no doubt as to the fact of a precipitate being formed, by mixing together an alkaline ſolution of ſilica and alumina. Alumina indeed appears to exerciſe an attraction, as I before

 ſtated,

ſtated, for ſilica, for magneſia, and for lime. All ſtones in which there is but little alumina, and a great quantity of ſilica, leave, after fuſion with potaſh, a light and flocculent ſubſtance, which cannot be diſſolved by the acids: this ſubſtance, however, which is ſilica, has been in ſolution in the alkali. But, if a greater proportion of alumina be preſent, none of this flocculent precipitate appears; hence it is evident, that alumina muſt determine its ſolution. Its eaſy ſolubility, in the latter caſe, cannot depend upon the diviſion of the particles of the ſilica in the ſtone; for, in the firſt place, after being fuſed with potaſh, the tenuity of the particles of every ſtone muſt be nearly the ſame; and, in the next place, I have not obſerved, that any earth, except alumina, can promote the chemical ſolution of the ſilica, though they muſt all occaſion its mechanical diviſion.

and promotes its ſolubility.

As to the affinity of alumina for magneſia, it is by much the moſt powerful of all thoſe which any of the earths have for each other. I attempted to precipitate magneſia from muriatic acid, by ammonia, even in exceſs; but found that the whole muriate of magneſia had not been decompoſed, and that a triple ſalt, or an ammoniacal muriate of magneſia *, had been formed. I then poured an exceſs of ammonia into a ſolution of muriate of magneſia, mixed with a large proportion of a ſolution of muriate of alumina. All the earth was precipitated; and nothing remained in ſolution, except muriate of ammonia. The liquor was then filtered, and the precipitate waſhed and dried. I diſſolved it in muriatic acid, and boiled it with a great exceſs of potaſh. Some alumina was taken up, but by no means all the quantity that had been uſed. The precipitate which had reſiſted the action of potaſh, was again diſſolved in muriatic acid, and precipitated by carbonate of potaſh. The carbonate of magneſia was held in ſolution by the exceſs of carbonic acid; and, by uſing potaſh and carbonic acid alternately, (the firſt to diſſolve alumina, the ſecond to diſſolve carbonate of magneſia,) I effected a ſeparation of the earths. Theſe experiments ſhew, that there is an affinity between alumina and magneſia, and a certain point of ſaturation, where the action of potaſh upon alumina is wholly counteracted by the affinity of that earth for magneſia.

Alumina very powerfully attracts magneſia.

* This ſalt is well known in chemiſtry.

When

Alumina attracts lime; which becomes dissolved in potash along with it.

When a solution of potash is boiled upon a mixture of lime and alumina, the alumina is dissolved, together with a much greater portion of lime than can be attributed to the dissolving power of the water alone. But, if a solution of potash be boiled upon lime, without alumina, no more lime is taken up than would have been dissolved by an equal quantity of water not containing potash in solution; consequently, alumina seems really to promote the solution of lime in potash. The affinity of alumina for lime, I had mentioned in the paper to which I allude; and it has since been noticed by Mr. Vauquelin *.

Guyton's conclusions, if true, would have rendered analysis uncertain.

If the conclusions of Mr. Guyton had been well founded, it would have been chemically impossible to arrive at truth in analysis. There were already real difficulties enough to be overcome; and Mr. Berthollet has lately discovered some, which are not so easily answered as those I have just considered. The position of this chemist, however, has been too generally extended by him. If the power of masses were as great as he represents it to be, and if it increased *ad infinitum*, in proportion to the mass, it must follow, that, with any given substance, we could decompose any compound, provided the mass of the decompounding body were sufficiently great; but this is well known not to be the case.

Objection to Berthollet's unlimited position respecting attraction from the mass.

From the experiments which I have related, it appears to be proved,

Recapitulation of the affinities of the earths, &c.

1st. That there exists an affinity between silica and alumina.

2dly. That there exists a very powerful affinity between alumina and magnesia.

3dly. That alumina shews an affinity for lime; but that the said affinity is not so strong as Mr. Guyton had supposed, nor, if pure reagents be used, is it to be perceived under the circumstances stated by him.

4thly. That Mr. Guyton was mistaken in every instance of affinity between the earths, excepting in the case of silica with alumina, which had been observed before his experiments; and that, in the other cases, he has attributed to a cause which does not exist, phenomena that must have resulted from the impurity of his reagents.

* Scheele was, in fact, the first who perceived this affinity. See his *Essay on Silex, Clay, and Alumina.*

5thly.

5thly. That neither the experiments of Mr. Guyton, nor the opinion maintained in the letter from Freyberg, are ſufficient to diminiſh, in any degree, the value of the aſſiſtance mineralogy derives from chemical inveſtigation.

IV.

A Memoir on the Muſical Sounds produced in Tubes by Hidrogen Gas. Read to the Society of Philoſophy and Natural Hiſtory at Geneva. By G. DELARIVE *.

IN a former meeting our learned colleague, Profeſſor Pictet, communicated to the Society a ſeries of experiments on the *muſical ſounds produced in tubes by hidrogen gas*, in which he developed the various muſical phenomena which theſe tubes produce. He explained the effect which the length or width of the tube, and the ſituation where the hidrogen is burned, have on the ſounds produced. As to the cauſe of the ſound, he gave only a few conjectures; his inquiries not being directed to that object. The purpoſe of the preſent memoir is to diſcover this cauſe. Profeſſor Pictet's reſearches on muſical tubes by hidrogen.

Profeſſor Brugnatelli is, I think, the firſt who publiſhed the experiment I have endeavoured to explain †: It was invented by a German. I ſhall here relate the principal circumſtances. Publication by Brugnatelli.

If a current of inflamed hidrogen gas be incloſed in a tube of an elaſtic and ſonorous material, ſuch as glaſs, metal, dry wood, &c. the tube after an interval of ſome ſeconds, will produce a muſical ſound. If it is open at both ends, the ſound will be ſtrong and full. It is poſſible however to ſucceed with a tube hermetically cloſed at one end, provided its diameter be large enough to allow of a circulation of atmoſpheric air ſufficient to keep the gas in a ſtate of combuſtion. The conditions eſſential to the ſucceſs of the experiment are, *firſt*, That the tube be elaſtic and capable of forming an echo, that is to ſay, capable of reflecting the undulations which proceed from the ſonorous point; for no ſound will be produced with a tube of paſteboard or paper: and, *ſecondly*, The flame muſt be produced by Deſcription of the manner of burning hidrogen in a tube, to produce a muſical ſound. The tube muſt be elaſtic; and the flame from hidrogen:

* Journal de Phyſique, LV. 165.

† Dr. Higgins diſcovered it. See Phil. Journal, new ſeries, I.

a current

a current of hidrogen; for an inflamed ſtream of the vapours of alcohol or ether, a lighted taper, &c. are incapable of producing any ſound.

Circumſtances.

Let us now examine the circumſtances of the experiment. There muſt be a point which may be called the *ſonorous point*; at which the vibrations, communicating an undulatory motion to the air, are produced. This point is the place of combuſtion; for if this be made to alter its ſituation, the ſounds will vary, as M. Pictet has proved in a ſeries of experiments. He has likewiſe obſerved, by means of ſmoke with which he filled the tube, a continual ſucceſſion of vibrations at this point; theſe vibrations produce the waves, which proceeding with a determinate velocity, ſtrike againſt the ſides of the tube, and are reflected with a velocity equal to that with which they arrived. When the diſtance of the ſides of the tube is ſuch that the alternate reflections are equal to the vibrations natural to the ſonorous cauſe, the ſound increaſes in intenſity, and becomes muſically appreciable. It appears likewiſe that the waves thus reflected do re-act on the primitive vibrations produced at the place of combuſtion, ſo as to render them harmoniouſly regular. For a certain ſpace of time is almoſt always neceſſary before the inſtrument acquires a regular and full ſound; and the tone or pitch of the tube will be more or leſs acute according to the greater or leſs number of undulations which take place in a given time.

The place of combuſtion is the ſonorous point.

Undulations at the point muſt accord with the reſonaut echo of the tube.

The ſound does not iſſue at firſt.

Its tone.

There is another fact eſſential to be obſerved in this experiment. The temperature of the column of air is not the ſame through all its length. At the ſonorous point, that is to ſay the place of combuſtion, the temperature is very high; ſo much ſo that the end of the adjutage of the glaſs through which the hidrogen paſſes, is conſtantly in a ſtate of ignition. If an inflamed ſtream of the vapours of alcohol or ether be ſubſtituted inſtead of the hidrogen, the heat is evidently leſs. From ſome experiments it alſo ſeems probable that the temperature of the room, and the purity of its air, may affect the ſucceſs of the experiment.

The temperature of the air in the tube is hotteſt at the burning point, and cooler more remote.

Alcohol or ether give a leſs degree of heat.

My experiments have been directed to aſcertain the cauſe of theſe phenomena, how and by what means theſe ſonorous vibrations are produced. We know that water is formed during the combuſtion of hidrogen: this water appears in the form of vapour. The temperature of the place of combuſtion being

Conjecture that the ſound is produced by the rapid production and collapſion of aqueous vapour.

being very elevated, these vapours must occupy an extensive space; but by immediately coming into contact with a cold air, their volume must rapidly diminish. A vacuum must thus be formed into which the air must collapse, and be again driven by other vapours, which again contract in their turn. Do not the vibrations result from these alternate motions, produced by the great expansion and subsequent contraction of the vapours *? Such were the conjectures that might be formed as to the probable cause of the phenomenon; but accident has presented me a fact which appears to give them some weight.

Sound produced by heating water in a thermometer bulb and tube.

I had a thermometer tube of about one line in diameter, at the extremity of which was a small bulb: there was a drop of water which I wished to expel from it; for this purpose I repeatedly exposed the bulb to the flame of a lamp with alcohol, and was agreeably surprised to hear a musical sound proceed from the tube.

Instructions for making the experiment.

In order that this experiment may succeed, a tube of one, two, or three lines in diameter, should be taken, of three, four, or five inches in length, with a bulb at one of the extremities, in diameter about three times that of the tube; it need not be very regular. If it were even a little flattened, I think the souud would be louder. A very small quantity of water or mercury must then be introduced, and expose it to a strong heat

The sound of air falling into a vacuum much more intense than that from explosion.

* It appears probable to me, that the sound produced by air precipitating itself into a vacuum, is more intense than that resulting from an expansive force. The horrible noise occasioned by the detonation of soap bubbles of hidrogen and oxigen is well known, though the lightest objects surrounding the bason are not even shaken. Whence it may be concluded, that the phenomenon is produced by the sudden vacuum proceeding from the destruction of the gases. The detonation of a pistol with inflammable gas is much greater than that of an air-gun, though the effect is less considerable; probably because a vacuum in the pistol succeeds the first expansive force. The child's toy, called the *humming top*, is well known. It is a hollow sphere, with an opening at its circumference: it produces a very strong humming noise when spinned rapidly on its axis. What may be the cause of this humming? I think it is the same as that which I have just mentioned; the centrifugal force drives the air from the sphere through the opening; a kind of vacuum is made, into which the external air continually presses, and is constantly driven back; whence proceed a series of sonorous oscillations.—D.

The humming top.

heat; that of a lamp with alcohol is commonly ſufficient; but the flame muſt be large and ſtrong when the tube is large. After the tube has been thus expoſed for a few ſeconds, the ſound is heard. Thoſe tubes whoſe diameters are conſiderable give a deep ſound, and the ſize of the bulb likewiſe ſeems alſo to contribute to the ſame effect: the ſound continues for ſeveral ſeconds, and then gradually decays, and at length entirely ceaſes. By ſuffering the apparatus to cool, and cauſing the condenſed liquid to deſcend into the bulb, the experiment may be repeated as often as ſhall be thought neceſſary.

Inveſtigation of both kinds of ſound.

Such is the experiment by means of which I think the phenomenon of the muſical tubes may be ſatisfactorily explained. Let us firſt examine what paſſes in the tubes with bulbs, with the eſſential conditions under which they produce their ſound, and then endeavour to diſcover the cauſe. I will then compare it with the ſound produced in tubes with hidrogen, and enquire in what reſpects the effects produced by theſe two inſtruments reſemble one another, with the differences they preſent, and the cauſe of thoſe differences.

Conditions for the tube and bulb.

There muſt be a bulb;

containing water;

or rather mercury;

but not ether, alcohol, or ſulph. acid.

The fluid muſt be very ſmall in quantity.

The heat muſt be ſtrong, to the bulb;

and the tube kept cold.

There muſt be ſome air in the tube.

The conditions eſſential to the production of ſound in tubes with bulbs are, *1ſt*, That the tube ſhould have a bulb. I never could excite ſonorous vibrations in a tube ſimply cloſed at one end. *2d*, This bulb muſt contain an evapourable liquid. Water ſucceeds very well, but it has this diſadvantage; it forms, in paſſing from the vapourous to the liquid ſtate, a drop of water in the tube, which often completely obſtructs it; and at other times, by running along the warm part of the glaſs, it frequently breaks it. Mercury has not this inconvenience: I never could ſucceed in producing ſounds with ether, alcohol, or concentrated ſulphuric acid. The ſounds are influenced by the quantity of liquid contained in the bulb. It ſhould be as ſmall as poſſible. If there be too much, the vapour fills the tube, and by completely expelling the air, renders it equally warm throughout, and no ſound can then be produced. *3d*, The third condition eſſential to the ſucceſs of this experiment, is the application of a ſtrong heat to the bulb while the reſt of the tube continues cold; for if there be not a marked difference in the temperatures of the tube and of the bulb, no ſonorous effect will take place. *4th*, The preſence of atmoſpheric air in the tube is indiſpenſable; if it be entirely excluded, no effect will be produced; and in every ſtage of the experiment it

it will be found that the vapour occupies only a certain part of the inftrument, and that air is always prefent. I have frequently endeavoured to determine the exact fpace occupied by the vapour at the moment the found was heard, and I found, at leaft in the fmall tubes, that it was nearly equal to the volume of the bulb. In order to determine this, I clofed the orifice of the tube with my finger at the moment it began to found; I then plunged the end of the tube in mercury, withdrew my finger, and fuffered the apparatus to cool. The vapour thus became condenfed, and the fpace it occupied was eftimated by the quantity of mercury which the preffure of the atmofphere caufed to rife in the tube. Proof of this.

Such are the four conditions effential to the production of founds; namely, a bulb at the extremity of the tube; the prefence of a very fmall quantity of water or mercury in this bulb; the application of a ftrong heat to it, whilft the remainder of the tube remains cold; and, laftly, the fimultaneous prefence of atmofpheric air and vapour in the apparatus; it is fcarcely neceffary to add, that the orifice of the tube fhould be always open. Let us now confider what may be the caufe of the found. I firft endeavoured to determine whether any chemical decompofition of the liquid employed took place. For this purpofe I ufed a tube fufficiently long to permit all the liquid to condenfe in it; I weighed it carefully before I fubjected it to experiment, and found that its weight was neither augmented nor diminifhed after repeated productions of found: Whence I conclude, that the caloric has no chemical effect on the liquid, but that it only experiences fucceffive evaporations and condenfations. Muft we then attribute the found to the evaporation and condenfation of the liquid? I thought fo at firft, but the following confiderations made me alter my opinion: I firft obferved that it was poffible to have a fucceffive evaporation and condenfation of the liquid, without producing found: this was effected by applying heat not fufficiently intenfe to produce found. Secondly, When the experiment was made with a drop of water, I always found that the moment when the apparatus began to found, was that at which the whole fluid had evaporated, and confequently that the caloric acts on the vapour alone. While the fmalleft atom of liquid water remained in the bulb, no found was produced. From this fact I have deduced, that the found is produced by the action

Recapitulation. The found is not produced by chemical change; nor by evaporation and condenfation; for thefe laft may happen without found;

action of caloric on the vapour and its re-action on the atmospheric air. I conceive that this phenomenon takes place in the following manner: The vapour contained in the bulb, by means of an addition of caloric applied on all sides and in great quantity, acquires an augmentation of volume and elasticity; it rapidly passes from the bulb into the tube, and drives out the air contained in it; but this air and the sides of the tube cause it again to lose a portion of its caloric, and its volume immediately diminishes. A vacuum is thus made, and the air resumes its primitive situation. Another addition of caloric restores its elasticity, which it speedily loses in the same manner. A series of oscillations by this process give the air an undulating motion; and the waves being reflected by the sides of the tube, become sonorous and appreciable as soon as their progress and recess are isochronous with the oscillations produced by the cause here pointed out. There are some tubes which never produce any sound: In this case I presume that the advance and recess of the waves cannot harmonize with the primitive oscillations, but that the one destroys the other. After a certain time the sound diminishes and dies away: This is explained by the propagation of the heat along the sides of the tube when the bulb is very warm and the tube cold. The vapour when first driven from the bulb, *suddenly* loses a part of its volume, and the oscillations thus produced are strong and frequent; but when the tube has acquired a certain degree of heat, the vapour then *gradually* diminishes in volume as it passes from a very high temperature to a situation certainly less warm, though of a sufficient degree of heat to cause the oscillations to become weaker, and at length entirely to cease. That this is the cause of the cessation of the sound, may be shewn by applying a strong heat to the part of the tube already warmed, and at the same time keeping up the first degree of heat at the bulb. By this means the limit of temperature is again precisely determined, and the sound reproduced in all its force. It may easily be conceived that the substance of the tube ought to be a non-conductor of heat, and on this account glass is preferable to any other substance.

but to a tremulous expansion and contraction of the vapour; which thus strikes the air and produces sound.

It seems as if the expansions of the vapour ought to be isochronous with the pitch or action of the tube governed by its length and diameter.

These sounds cease as soon as the tube has become too hot to occasion the sudden contraction of the vapour.

Experiments in confirmation.

Let us now compare the tubes which sound by means of hidrogen with those with bulbs. In the former we have every condition necessary to the production of sound; a very hot vapour, and consequently very elastic; for as we observed, the

Concerning the sounds produced by hidrogen. Repetition of the theory already stated.

the period of combuſtion is at ſo high a temperature, that it always ignites the tip of the glaſs tube: this hot elaſtic vapour, at the moment of its production, comes in contact with the cold air, which enters through the bottom of the tube and goes out at the top. Its volume muſt be diminiſhed the inſtant it ſtrikes this air; other heated vapours ſucceed the former, and are alſo contracted: this alternate expanſion and contraction produces the undulatory motion of the air, and cauſes the ſonorous waves.

Expanſions and collapſions of aqueous vapour near the point of combuſtion.

We have found that an inflamed ſtream of alcohol or ether produced no ſound in a tube. This is a proof of my obſervation, that in order to produce ſound, there muſt be a great difference in the temperature of the vapour and the ambient air. There is certainly, in this caſe, a formation of vapours and ſucceſſive condenſations, for the water runs along the ſides of the tube; but the point of combuſtion is infinitely leſs hot than that of hidrogen, conſequently the vapour produced is much leſs heated and elaſtic. This caſe is ſimilar to that formerly mentioned, where it was ſhewn that vaporization and ſucceſſive condenſation of the liquid might be effected without ſonorous undulation, merely by expoſing the bulb to a certain degree of heat not intenſe enough for this laſt purpoſe. We cannot be ſurpriſed that there is leſs heat produced by the combuſtion of alcohol or ether than by that of hidrogen, if we conſider, that in the latter caſe all the caloric contained in the gas and the atmoſpheric oxigen which is conſumed, becomes ſenſible heat, and is paſſed totally into the vapour produced. On the contrary, in the combuſtion of an inflammable ſubſtance, ſuch as alcohol, the caloric of the oxigen conſumed alone becomes ſenſible, and this alſo is moſtly abſorbed by the production of carbonic acid gas; ſo that the exceſs alone paſſes into the vapour. It is not therefore ſurpriſing that the heat is not ſufficient to give the vapour elaſticity ſufficient to produce ſounds. The preſence of carbonic acid gas, which is one of the reſults of the combuſtion, may alſo prevent the ſonorous vibrations.

Alcohol and ether burn with leſs heat, and produce elaſtic fluid which is neither rapidly nor totally condenſed.

Why there is leſs heat.

The ſound is much ſtronger in the tubes with hidrogen, than in thoſe with bulbs. It is likewiſe more permanent; for the tube being open at both ends, a current of cold air conſtantly enters at the bottom and iſſues out at the top: this current of air takes up and carries off the hot elaſtic vapours, receives their impulſe, and by attracting a portion of their caloric, diminiſhes

The ſound by hidrogen is ſtrong and permanent, becauſe the conſtant ſupply of cold air produces the condenſations inceſſantly.

minifhes their volume. This then is the moft effential condition to the production of an intenfe permanent found; namely, a great difference between the temperature of the air and the vapour; and this difference remains always the fame, on account of the continual renewing of the air. The fame effect does not take place in the tubes with bulbs, and accordingly their found is weaker and lefs durable.

Not fo in the bulbs.

The founds by hidrogen are lefs intenfe in a warm room filled with company;

From the knowledge of this condition, that a great difference between the temperature of the air and the vapour is neceffary to the production of found, it will be eafy to comprehend that every circumftance which augments the heat of the current of air, and diminifhes that produced by the combuftion of the gas, will tend to weaken and even to annihilate the found of the tube. Thefe two circumftances are both found in a warm room filled with company. The current of air inftead of being cold is heated, and the quantity of oxigen being lefs, the heat of the combuftion is lefs. It is not then furprifing that in fuch rooms the experiment does not always fucceed.

becaufe the air is warmer, and the combuftion weaker for want of oxigen.

Sounds by phofphorus.

Brugnatelli produced founds by the fimple combuftion of phofphorus in tubes. Some philofophers being perfuaded that fonorous effects could only be attributed to hidrogen, were difpofed to infer its prefence in phofphorus. From what we have ftated, it feems more fimple to explain the phenomenon by the production of phofphoric acid in the vaporous form, which becomes ftrongly elaftic by means of the caloric difengaged during the combuftion, but of which the volume diminifhes by the contact of cold air. We have here the alternate expanfion and contraction neceffary to the production of found.

Conclufion.

Such are the fmall number of obfervations which I have had an opportunity to make on the fonorous tubes. I hope that they will intereft fuch as are more particularly engaged in this branch of natural philofophy, and tend to direct their attention to a curious fact which has not hitherto been fufficiently examined.

ANNOTATION.—W. N.

That aqueous vapour is condenfed with great rapidity;

AS the fundamental effect in this very ingenious explanation (namely, that aqueous vapour is condenfed with extreme rapidity), may not appear to fome readers to be fo

so speedy as to produce sonorous undulation, I think it will not be impertinent to mention a few facts relating to that subject. In the small glass instrument called the water-hammer, which is a tube of about three quarters of an inch in diameter, with a ball of about an inch and half at one end, the other end being hermetically closed; the ball contains water, and the empty space is rendered nearly vacuous by boiling the fluid previous to sealing it. In this instrument the heat of the hand, applied to the wetted tube, is sufficient to produce bubbles of vapour which enter the ball, but speedily collapse. The series of these condensations is as quick as fifteen or sixteen in a second. But in the steam engine the condensation is prodigiously more rapid. I have a small double steam engine on the construction of Boulton and Watt, having all the parts and gear of the large engines, but its cylinder is only $2\frac{1}{2}$ inches diameter, and the length of stroke $6\frac{1}{4}$ inches. When this was set to work the other day, in a lecture to my pupils, it gave 600 * strokes per minute. By an easy calculation it will be shewn, that the steam condensed was then much more than 300 cubic inches per second; and if the condensation, instead of being effected in masses of about a pint at a time, could have been performed by successive collapsions of each cubic inch in an open space, the pulses would have produced the tone of the lowest E flat in the treble cliff. But the number of cubic inches condensed in a large steam engine, for example, a three feet cylinder with an eight feet stroke, will be eight or nine times as much, at the usual rate of working.

shewn by the water-hammer,

and by the steam engine.

A small steam engine working twice as quick as the balance of a watch; and condensing ten pints of steam in a second.

This condensation, if by successive inches, would give a very loud E flat.

* About twice as many as the beats of a common watch.

V. *Account*

V.

Account of a ſimple Method of eſtimating the Changes of Volume produced in Gaſes, by Alterations of Temperature, and of Atmoſpheric Preſſure, in the courſe of Chemical Experiments. By MR. H. DAVY. *Profeſſor of Chemiſtry at the Royal Inſtitution. From their Journal.*

Elaſtic fluids affected by preſſure and temperature.

IT often happens that changes of atmoſpherical preſſure, and of temperature, take place in the courſe of experiments on elaſtic fluids; and a knowledge of the alterations they produce in their volume, is eſſential to the preciſion and accuracy of the reſults.

Quantities of gaſes muſt be eſtimated at ſome ſtandard.

In caſes when chemical changes are produced, it is impoſſible to gain this knowledge by direct obſervation; and, in conſidering quanties, it is always uſeful to eſtimate the volumes of gaſes at ſome ſtandard, fixed upon in meaſures of the barometer * and thermometer.

Law for preſſure. Volume, inverſely as preſſure.

It is demonſtrated by very accurate experiments, that the volumes of elaſtic fluids are inverſely as the weights compreſſing them. And, conſequently, the changes produced upon gaſes, by known changes in the atmoſpherical preſſure, may be aſcertained in a very eaſy manner. With regard to the effects of temperature, however, it is much more difficult to form a juſt eſtimation by means of general laws. For though the excellent experiments of Mr. Dalton †, and thoſe of

* Lavoiſier's Elements, p. 406, 2nd Edit.

† Mancheſter Memoirs, Vol. V. p. 599. Mr. Dalton ſays, "I have repeatedly found that 1000 parts of common air, of the temperature of 55° and common preſſure, expand to 1321 parts in the manometer; to which adding 4 parts for the correſponding expanſion of glaſs, we have 325 parts increaſe upon 1000 from 55° to 212°; or for 157° of the thermometric ſcale. As for the expanſion in the intermediate degrees, which Col. Roi's experiments ſhow to be a *ſlowly diminiſhing* one above the temperature of 57°, but which de Morveau's, on the contrary, ſhow to be a *rapidly increaſing* one in the higher part of the ſcale; I am obliged to allow that Col. Roi is right, though it makes, in ſome degree, againſt an hypotheſis I have formed relative to the ſubject; he has certainly, however, made the diminution too great from 72° downwards, owing

of Mr. Gay Luſſac, ſhow that all the different elaſtic fluids, taken at equal temperatures, expand equally by heat, yet we are ſtill ignorant of the preciſe expanſion for increments of ſingle degrees, or of the mode in which the power of expanſion is affected by difference of preſſure; or by its relations to aqueous vapour or uncombined moiſture.

The calculations on this ſubject are conſequently laborious and complicated; but it is very eaſy to avoid them, by recurring to comparative obſervations, which may be obtained in a very ſimple manner, by means of the manometer. Calculation may be avoided by comparative obſervations,

By making a given quantity of air, in contact with water at a known temperature and preſſure, a ſtandard, it is eaſy, in aſcertaining the changes produced in it by alteration in the temperature and preſſure, to determine, by the rule of proportion, the changes that have been produced from the ſame cauſes in any other quantity of gas, ſubmitted to chemical operation; or to aſcertain what would be its volume at the mean height of the barometer and thermometer. with a ſtandard quantity of common air,

owing to his not perceiving that he actually deſtroyed a portion of the elaſtic fluid he was operating upon (aqueous vapour) in reducing its temperature ſo low; if his air had been previouſly dried by ſulphuric acid, &c. he would not have found ſo remarkable diminution below 72°. My experiments give for 77°½ above 55°, 167 parts; for the next 77°½ only 158 parts: and the expanſion in every part of the ſcale ſeems to be a gradually diminiſhing one in aſcending.

" The reſults of ſeveral experiments made upon hydrogenous gas, oxygenous gas, carbonic acid gas, and nitrous gas, which were all the kinds I tried, agreed with thoſe on common air, not only in the total expanſion, but in the gradual diminution of it in aſcending: the ſmall differences obſerved never exceeded 6 or 8 parts on the whole 325: and differences to this amount will take place in common air, when not freed from aqueous vapour, which was the ſituation of all my factitious gaſes.

" Upon the whole therefore I ſee no ſufficient reaſon why we may not conclude, that *all elaſtic fluids under the ſame preſſure expand equally by heat*—and that *for any given expanſion of mercury, the correſponding expanſion of air is proportionally ſomething leſs, the higher the temperature.*"

Mr. Gay Luſſac makes the dilatation to be from 100 to 137.5 between 32° and 212°, which gives for each degree between theſe points $\frac{1}{493}$. See p. 134; and Annales de Chimie, No. 128, p. 137.

confined in a recurved tube by water in the bend;

In forming the manometer for the purpofe of comparifon, a glafs tube may be ufed about fixteen inches long, and one third of an inch in diameter, clofed at one end, and curved in fuch a manner that its open leg is parallel to its clofed end, and nearly three inches long. Its capacity is determined, and its clofed leg is graduated fo as to form a fcale of 200 parts. The ftandard volume of air is confined in it by a column of water, four inches long; and the height of this water is kept equal in both legs at the different times of obfervation, by means of a glafs tube, moveable in a perforated cork inferted in the open end, and capable of elevating the column of water in it at leaft an inch and a half.

graduated

the water kept level, by a plunger.

Ufe of the ftandard of comparifon.

In employing the ftandard of comparifon, for correcting the refults of operations in which portions of elaftic fluids are either abforbed or generated, it is only neceffary to recur to the ftate of it at the beginning and end of the experiment. Thus, let n equal the quantity of elaftic fluid exifting after the experiment; m the volume of the ftandard air before the experiment; and v the volume after, as expreffed in the fcale of 200 parts.

Inftance.

Then $\frac{nm}{v} = x$, which is the volume the refidual gas examined would occupy at a temperature and preffure fuch as exifted at the commencement of the experiment.

By the fame method may be eftimated the volume a quantity would occupy at fuch temperatures and preffures, as had been at any time denoted upon the fcale.

It is moft probable that the refults are not difturbed by the aqueous vapour.

From the lateft experiments that have been made *, it is probable that at the fame temperatures, and under the fame preffures, equal volumes of the different elaftic fluids, in contact with water, contain the fame quantity of aqueous vapour; fo that in cafes when the gafes examined in the comparative obfervations are equally faturated with water, the refults muft be perfectly accurate as to the relation of volume to the ftate of moifture; and, even fuppofing a difference in the degree of faturation, the error arifing from this circumftance, at common temperatures, would be fo fmall as to be inappreciable.

* Thofe of Sauffure and De Luc, and MM. Deformes and Clement.

VI. *Obfervations*

VI.

Observations on Sir George Shuckburgh Evelyn's Paper * *in the Philosophical Transactions for* 1798, *on the Standard of Weight and Measure. By* J. FLETCHER, *Esq. Communicated by the Author.*

I. HAVING about two years ago had occasion, in the course of some investigations in which I was then engaged, to ascertain with as much precision as was possible the relation between the known measures of distilled water and their respective weights at different temperatures; Sir George Shuckburgh Evelyn's Paper on this subject was of course considered as the best authority with regard to what had been previously done in this respect. I found however, on looking into it, some errors in the calculations, which render the deductions from them incorrect, at the same time that they more strongly confirm and establish the accuracy of the experiments on which they are founded. I have no doubt, therefore, that the gentleman to whom we are so highly indebted for the communication of the facts which he states in this important essay, will feel rather obliged than otherwise by their correction. The circumstance which first induced me to repeat the calculations, was the difference which there appears to be in the results of the experiments with the cube and sphere, and that with the cylinder, and which Sir George ascribes, in § 24 and 31 of his Paper, to either a difference in the density of the water at different depths, or to the compressibility of the cylinder employed. I was however as much pleased as surprized to find, that it altogether arises from a mistake in the multiplication of the four numbers whose product is equal to its solid content, *viz.* 3.99745 × 3.99785 × 5.99502 × .78539816, which he makes 74.94823 cubic inches, instead of 75.247149, which is the real product. This naturally suggested the propriety of examining the whole, and the results of this examination are what I shall now lay before the public through the medium of the Philosophical Journal. The power of correctly appreciating and comparing space and quantity are so essential to every

Occasion of this essay.

Numerical errors in Sir G. S. Evelyn's paper.

Apparent difference in results.

Mistake in the solid content of his cylinder.

* Copied in the Quarto Series of this Journal, III. 197.

every practical application of the mathematics to the purposes of real utility, that I have no hesitation in undertaking the humble office of commentator on so valuable a text.

Real cubic dimensions of Sir G. S. Evelyn's solids.

II. It is stated in § 13* that the product of the three mean measures of the sides of the cube, *viz.* $4.98882 \times 4.98955 \times 4.98925$, is 124.18917: this however is erroneous, it being in fact 124.192246. The solid content of the cylinder was 75.247149, as already stated in the preceding paragraph, and that of the sphere (§ 28) $= \overline{6.00745}^3 \times .52359878 = 113.519147$ cubic inches.

Correction for temperature of the brass.

III. Supposing the brass scale and these bodies to be at the same degree of heat, their apparent dimensions as measured by it would be the same whatever was the temperature of both; the scale being of the same substance, and consequently expanding and contracting with the bodies themselves. Their real dimensions however varying with the temperature, we must assume some certain degree of heat at which the graduations on the scale are to be considered as measuring correspondent intervals of space. We will suppose this to be 60° of Fahrenheit's thermometer. Now in order to ascertain the real cubic measure by the scale when at 60°, of the quantities of distilled water displaced by those bodies when they were weighed in it, we must add to or subtract from the nominal cubic dimensions of each, 3 millionth parts (vide Tab. I. in the note on § 23), multiplied by the number of degrees above or below 60°, at which they and the fluid were at each of those times.

At the time of their being respectively weighed in water, therefore, the solid content of the cube at 60.2° (§ 22) was $124.192246 \times 1.00000062 = 124.192323$ cubic inches of the scale when at 60°; that of the cylinder at 60.5° (second experiment, § 23) $75.247149 \times 1.0000015 = 75.247161$; and that of the sphere at 66° (§ 29) $113.519147 \times 1.000019 = 113.521304$.

Correction necessary for density and temperature of the air.

IV. In appreciating the apparent difference of weight in a body when weighed in air or in vacuo, it has been usual to assume, that air at a given temperature, and under a given

* These references with a § prefixed, are to the sections so numbered in Sir G. S. Evelyn's Paper, which it is thought proper to mention here to save the necessity of repetition.

 pressure

preſſure, is always of the ſame ſpecific gravity. This we know is not the caſe; conſiderable variations in this reſpect being occaſioned by its different ſtates of dryneſs, electricity, and perhaps other cauſes: as, however, experiments are wanting for the correct eſtimation of theſe differences, we will take the firſt ſuppoſition to be true, and apply the corrections accordingly from Sir G. S. Evelyn's tables for this purpoſe, in his note ſubjoined to § 23.

Air expands .0027 for 1° of Fahrenheit.

This gentleman tells us in the Philoſophical Tranſactions for 1777, that the ſpecific gravity of air at 51°, under a preſſure equal to 29.27 inches of mercury, is to that of water as 1 to 836, or = .001208; taking the latter as unity. Aſſuming therefore that the expanſion of air, for each degree of Fahrenheit's thermometer between 51° and 60°, is .0027 of the whole, we get the ſpecific gravity of air at 60 under the like preſſure = .001179, and 29.27 : .001179 : : 29.5 : .001188 = the ſpecific gravity of air at 60° when the barometer ſtands at $29\frac{1}{2}$; and a cubic inch of ſuch air conſequently weighs $\frac{3}{10}$ of a grain very nearly.

Sp. gr. of air at 60°, bar. 29.5, = .001188 = .3 gr. for each cubic inch.

Denſities of the air in which the bodies were weighed.

Now the cube was weighed in air at 62° under a preſſure of 29 inches (§ 20); the cylinder in air at 62° under a preſſure of 29 inches (§ 21); and the ſphere in air at 67° under a preſſure of 29.74 inches (§ 29).

And the exceſſes of the weights of the reſpective quantities of diſtilled water diſplaced by theſe bodies, on their being afterwards weighed in this fluid, over thoſe of ſimilar quantities of air at the ſeveral temperatures and denſities above-mentioned, were as follow; *viz.*

The apparent weights of equal bulks of water.

That of the water diſplaced by the cube (§ 22), 32084.82 − 703.03 = 31381.79 grains;

That of the water diſplaced by the cylinder (ſecond experiment § 23), 21560.05 − 2553.22 = 19006.83 grains;

And that of the water diſplaced by the ſphere (§ 30), 28673.51 grains.

Correction for preſſure and temperature applied.

The air in which each of the bodies was weighed being, however, lighter than air at 60° when the barometer ſtands at $29\frac{1}{2}$, we muſt correct theſe exceſſes of weight in the ſeveral quantities of water, ſo as to find how much the weight of each of them exceeds that of an equal quantity of air of ſuch ſtandard ſpecific gravity; or in other words, how much the weight of each of the bodies in water differs from that which

it

it would have been found to poſſeſs in air in this latter ſtate. Theſe corrections for the denſity and temperature of the air we ſhall find, from the data before given in this article, to be as follows:

	Correction for Preſſure.	Correction for Temper-ature.
	Gr.	Gr.
Of the weight of the water diſplaced by the cube - - -	− .642	− .201
Of the weight of the water diſplaced by the cylinder - - -	− .389	− .122
Of the weight of the water diſplaced by the ſphere - - -	+ .282	− .644

Correction of ſp. gr. of water when reduced to 60°.

The weights of theſe bulks of water, therefore, if weighed in air at 60° under a preſſure of $29\frac{1}{2}$, would be found to be,

That diſplaced by the cube 31381.79 − .642 − .201 = 31380.95 grains;

That diſplaced by the cylinder 19006.83 − .389 − .122 = 19006.32 grains;

And that diſplaced by the ſphere 28673.51 + .282 − .644 = 28673.15 grains.

V. We have now, however, to correct theſe weights for the change which would take place in the ſpecific gravity of the water itſelf, by reducing it to 60° of temperature; that in which the cube was weighed having been at 60.2° (§ 22); that in which the cylinder was weighed at 60.5° (§ 23); and that in which the ſphere was weighed at 66° (§ 30).

Firſtly, by Sir G. S. Evelyn's table.

We will firſtly do this by the help of Sir G. S. Evelyn's Tab. I. in his note on § 23, according to which, a quantity of water of equal bulk with the cube would, at 60° of Fahrenheit's thermometer, weigh 31380.95 × 1.000033 = 31381.986 grains; one of equal bulk with the cylinder, 19006.32 × 1.000083 = 19007.897 grains; and one of equal bulk with the ſphere, 28673.15 × 1.00099 = 28701.536 grains; if weighed in air at the ſame temperature, when the barometer ſtands at 29 inches and a half.

The

Refult.

	Grains by Sir G. S. Evelyn's Weights, or	Grains of the Parliamentary Standard Weights, which are thofe ufed by him as 1.0006562 : 1. (§ 41).
The firft gives the weight of a cubic inch of diftilled water under thefe circumftances $= \frac{31381.986}{124.192323} =$	252.689	252.523
The 2d $= \frac{19007.897}{75.247161} =$	252.606	252.440
And the 3d $= \frac{28701.536}{113.521304} =$	252.829	252.663

VI. It may perhaps however be proper to obferve here, that the alteration in the fpecific gravity of water by change of temperature, is eftimated in this table of Sir G. S. Evelyn's (note on § 23), at nearly double that which is given by Mr. Gilpin in his tables, in the Philofophical Tranfactions for 1794, p. 382; his appreciation of it from 60° to 66° (taking it to be equal to unity at 60°) being as follows; *viz.* By Mr. Gilpin's tables;

61°	.99991	63°	.99971	65°	.99950
62	.99981	64	.99961	66	.99939

I am, from my own experiments on this fubject, difpofed to prefer thefe tables; and taking them to be correct, we fhall have a cubic inch by Sir G. S. Evelyn's fcale when at the temperature of 60°, of diftilled water alfo at 60°, weighed in air at 60°, under a preffure of $29\frac{1}{2}$ inches.

Ultimate refult. A cubic inch of water at 60°, in air at 60°, bar. 29.5, = 252.506 grains;

		Grains of the Parliamentary Standard according to Sir G. S. Evelyn's appreciation of it in § 41 of his Paper.
By the cube = - -	252.519	
By the cylinder = - -	252.432	
By the fphere = - -	252.568	
And by a mean of all three =	252.506	
And if weighed in vacuo inftead of in air = - -	252.806	

or = 252.806 grs. in vacuo;

J. FLETCHER.

Cecil Street, Dec. 20, 1802.

VII. *On*

VII.

On the Quantities of Light afforded by Candles in Proportion to the Consumption of Material and other Objects respecting the same. In a Letter from MR. EZ. WALKER.

To Mr. NICHOLSON.

SIR,

Probability that Candles which burn without producing smoke, will afford light in proportion to the matter consumed.

WHEN a lighted candle is so placed as neither to require snuffing nor produce smoke, it is reasonable to conclude that the whole of the combustible matter which is consumed is converted to the purpose of generating light; and that the intensities of light generated in a given time by candles of different dimensions, are directly as the quantities of matter consumed. That is to say; when candles are made of the same materials, if one candle produce twice as much light as another, the former will in the same time lose twice as much weight as the latter.

Experiments in proof.

To prove the truth of this position, I made the experiments contained in the following

TABLE.

No. of the Experiment.	No. of the Candles.	Time of burning.	Weight of the Candles consumed.	Strength of Light.	Distance of the Candles from the Wall.
		h. ′	oz. dr.		Feet.
1	1	3 0	0 15	1.	7
	3	3 0	1 1½	1. +	7
	Mould	3 0	0 15	1.	7
2	1	2 55	0 15	1	8
	3	2 55	1 0	1 +	8
	Mould	2 55	0 15	1	8
2	1	3 0	0 15¾	1	8
	3	3 0	1 2	1⅛	8¾
	Mould	3 0	1 0	1	8
4	5	3 0	1 5	1.18	8¾
	Mould	3 0	1 1⅛	1.	8

These

These experiments were made in the following manner.

I took three candles, the dimensions of which are given in the table, against 1, 3, and mould *. These were first weighed, and then lighted at the same instant. At the end of the time inserted in the third column of the above table, they were extinguished and weighed again, and the loss of weight of each candle is contained in the fourth column. Method of experiment.

The three first experiments were made under such favourable circumstances, that I have little doubt of their results being more accurate than what practical utility requires, but the fourth experiment cannot be depended on so much, in consequence of the variable light of No. 5. This candle was moved so often to keep the two shadows equal, that I was under the necessity of setting down its mean distance from the wall by estimation; but as this was done before the candles were weighed, my mind could not be under the influence of partiality for a system. Observations on the results.

The method which I used in comparing one light with another in each experiment, was nearly that which has been already described †. The only difference consisted in having the obstacle which formed the shadows fixed, instead of being held in the hand. A two feet navigation scale, which was made fast to the stand of a small telescope in a perpendicular direction, and set upon a table near the wall on which the shadows were compared, reduced the labour of making the experiments very much, and gave me an opportunity of making a greater number of observations in the same time than I could have done with the scale in my hand. Intensity of light determined by the method of shadows.

REMARKS ON THE EXPERIMENTS.

1. The experiments were made at different times, and the light of the mould candle was made the standard, with which the lights of the others were compared; but it must not be understood, that this candle gave the same strength of light in every experiment. The standard comparison of experiments at different times was a mould candle by the weight consumed, of which the lights were infered.

2. The sign + in the 5th column, signifies that the candle against which it is placed, gave a stronger light than the others Other remarks.

* Philosophical Journal, octavo, III. 273.

† Philosophical Journal, quarto, I. 67. and Philos. Journal, octavo, III. 275.

in the ſame experiment, but the exact quantity being ſmall, I did not aſcertain by calculation.

3. The mould candle in the 4th experiment, loſt more of its weight in three hours, than in the preceding experiments. This was owing to the current of air in the room in which the experiments were made, being greater than uſual, occaſioned by a freſh gale of wind.

GENERAL LAWS.

General law as at firſt ſtated.

From the experiments contained in the table, it appears to be an eſtabliſhed law, where combuſtion is complete, that the quantities of light produced by tallow candles, are in the complicate ratio of their times of burning and weights of matter conſumed.

For if their quantities of matter be equal, and times of burning the ſame, they will give equal quantities of light, *by the experiments.*

And if the times of burning be equal, the quantities of light will be directly as their weights of matter expended, *by expements 3 & 4.*

Therefore the light is univerſally in the compound ratio of the time of burning and weight of matter conſumed.

STANDARD OF LIGHT.

More ample explanation reſpecting the ſtandard candle.

If the law which I have endeavoured to prove, both by reaſon and experiment, be admitted, we have a ſtandard by which we may compare the ſtrength of any other light.

Let a ſmall mould candle, when lighted, be ſo placed as neither to produce ſmoke nor require ſnuffing, and it will loſe an ounce of its weight in three hours. Let this quantity of light produced under theſe circumſtances, be repreſented by 1.00.

Then ſhould this candle at any other time, loſe more or leſs of its weight in three hours than an ounce, the quantity of light will be ſtill known, becauſe the quantity of light in a given time is directly as the weight of the candle conſumed, *by the general law.*

CONCLUSION.

Advantages of the inclined candle;

A candle which is uſed in the manner that I have pointed out, gives *more light* than a candle of the ſame dimenſions ſet

 perpeudicularly

perpendicularly and ſnuffed, becauſe one part of a candle that is ſnuffed is thrown away, and another part flies off in the form of ſmoke. But this is not the only inconvenience that attends the uſing of a candle in this manner, and which the other method is free from, for the light which it gives is of a bad quality, on account of its being variable and undulating.

From the time that a candle is ſnuffed till it wants ſnuffing again, its ſtrength of light ſcarcely continues the ſame for a ſingle minute. But that variation which frequently takes place in the height of the flame, is a matter of ſtill more ſerious conſequence.

contraſted with the upright candle, which requires ſnuffing.

The flame of a long candle, when it burns ſteadily, is about two inches high, but it very frequently riſes to the height of four inches or upwards; drops down again in a moment, till it is leſs than three inches, and then riſes again. In this manner the flame continues in motion for ſome time before it returns to its original dimenſions. But it does not continue long in a quieſcent ſtate before it begins a new ſeries of undulations. In this manner the candle burns till the top of the wick is ſeen near the apex of the flame, carrying off clouds of ſmoke. In this ſtate of things the eye becomes uneaſy for want of light, and the ſnuffers are applied to remove the inconvenience.

Extreme undulations of large candles placed vertically;

It is theſe ſudden changes, and not the nature of candle light itſelf, that do ſo much injury to the eye of the ſtudent and artiſt; but that injury may be eaſily prevented, by laying aſide the ſnuffers, and in the place of one large candle, let two ſmall ones be uſed in the manner which I have before taken the liberty to recommend.

and the miſchievous conſequences to the eyes.

I remain,

SIR,

Your's reſpectfully,

EZEKIEL WALKER.

Lynn Regis, Dec. 20, 1802.

Deſcription

VIII.

Description of an Engine for raiſing and lowering Weights by the Action of a Column of Water and for other purpoſes. *By* JOHN HARRIOTT, *Eſq.*

To Mr. NICHOLSON,

Dear SIR,

I SEND the drawings and deſcriptions of my ſyphon engine, which from its power and great convenience in actual practice, will I preſume be thought a fit ſubject for your valuable publication.

Engine acting by a column of water acting alternately on the upper and under ſurface of a piſton, as directed by turning a cock.

A A. In Fig. 1 and 2, is a cylinder with a moving piſton therein, of which D is the piſton rod.

B and C. Are water ways through which the water is admitted to communicate with both ſides of the piſton.

E F. A pipe in Fig. 1, through which water deſcends from a reſervoir above, into a three way cock M. and in Fig. 2, is a pipe through which any ſtream or head of water runs to the three way valve in the ciſtern M.

C H. Is a pipe in both, communicating from the three way cock, or valve, to the upper part of the cylinder.

K B. Is a pipe communicating from the ſame cock or valve, to the lower part of the cylinder.

I I. Is a pipe communicating between the two laſt mentioned pipes, conſequently between the upper and lower ſpaces of the cylinder, which communication can be either cut off or opened to any requiſite degree by the cock L.

N. Is a pipe in which a lower column of water is ſuſpended by the reaction of the atmoſphere, and conſequently a power to the upper column, or fall in proportion to its length or depth, not exceeding the weight of the atmoſphere.

The column conſiſts partly of an actually preſſing maſs and partly of a ſuſpended portion, which renders the atmoſphere active on the machinery.

The nature and principle of the ſyphon engine conſiſts in combining the power of the ſyphon with the direct preſſure of a column or ſtream of water, ſo that they may act together. It works by means of the ſyphon conſtantly acting in concert with the feeding ſtream of water, ſo that each alternately act on the upper and lower part of a piſton, within a cylinder as it were, reverſing the ſyphon at each change; and the power is equal to a column of water of the ſame diameter as that of the

the cylinder, and equal in length to the height of the head, above the tail water. For inſtance, if a column of water of any given diameter has a fall of 20 feet until it reaches an engine, its power is clearly aſcertained. Now whatever that power is, if a ſyphon pipe be added to this engine, ſo as to connect with the column, and the ſyphon pipe has alſo a fall of an equal length; namely, 20 feet to the lower end, which is immerſed in water, the engine although placed in the midway, will then have a power equal to that of a deſcending column of 40 feet, and ſhould the column or fall to the engine be but two feet, and the lower ſyphon pipe 24 feet, the power would be equal to a fall of 26 feet; and in this manner in every various diverſity between the falling column and the ſyphon pipe beneath, the latter will produce an equal power according to its proportionate length, or depth to the ſurface of the tail water, provided it does not exceed above 30 feet, or the weight of the atmoſphere; and where a ſtream of water is either level with, or even below, the place at which it is deſirable to fix the engine, there will be no difficulty in placing it either below, or on the level, or above the ſtream itſelf, provided the height where it is fixed above, does not exceed 28 or 30 feet, and the place where the water flows off be ſtill lower. The conſtruction may evidently be varied according to the local ſituation and circumſtances of applying it, and the uſe to which it may be adapted, in giving activity to different kinds of machinery.

Engine to be uſed inſtead of a crane. Its advantages ſtated.

The drawing, Fig. 1, exhibits the apparatus for raiſing or lowering weights of any kind, on wharfs or in warehouſes. A man or boy can raiſe or lower goods of any weight, without other exertion than that of merely turning the three way cock M. to an index; in either raiſing or lowering, the *ſtop* is inſtantaneous, by a ſmall motion or turning the cock to the ſtop mark in the index: this moſt effectual of ſtops, or gripe, operates ſo quietly and eaſy without any jirk, or jarring, that it removes the uſual riſk attending common cranes or machinery in which men are ſometimes overpowered. It raiſes and lowers goods with thrice the velocity uſually produced by manual labour, yet an engine of dimenſions ſufficient to raiſe ſeveral tons, may be ſo graduated by the perſon at the cock, as to bring it to the ſmootheſt floweſt motion poſſible. The ſaving of labour and time muſt therefore be conſiderable, the

riſk

riſk of plunder diminiſhed, and delays in ſetting to work for want of help removed.

Though this engine requires a reſervoir of water; yet this may eaſily be had and will be highly beneficial in caſes of fire.

The great and only obſtacle to its general application in this way, is the want of a natural head of water in moſt ſituations of warehouſes; and the queſtion in theſe caſes would be, whether it would be worth while to raiſe a head or reſervoir of water for ſuch purpoſes. If this be done, *one* reſervoir would work any number of engines at any diſtance by one main pipe, and as many branch pipes as there were engines. The expence of a ſmall ſteam engine to raiſe the ſame water (or any other) to the reſervoir, would be more than repaid by the ſecurity it would give againſt fire. A gallon of water at the firſt alarm of fire, has more effect in extinguiſhing it, than a hogſhead after it has got a-head. To produce this effect, and apply it in the ſpeedieſt manner wherever ſuch reſervoir is, would only require ſmall pipes to be led from the main pipe that ſupplies the engines, to the warehouſes, or where it may otherwiſe be wanted, and at the end of ſuch pipe, a ſcrew nozzle and a cock to turn on or off, a leather hoſe and branch pipe, ſuch as all fire engines have, being provided and hanging near. One man only at the firſt alarm of fire, would in a few minutes ſcrew the hoſe upon the ſcrew nozzle, and then turning the cock, he would of himſelf be able to play upon the fire with all the force of a ſtrong fire engine.

Propoſal for public reſervoirs of water in every pariſh.

Perhaps it is not too much to ſay, that this ready preventative in caſe of fire, would be equal in effect to an inſurance. Independent therefore of its application to the ſyphon engine for raiſing and lowering weights, I have conceived, that if ſuch reſervoirs of water were provided in all cities and towns, well elevated at ſuitable diſtances (one or more in every pariſh) there would not be near the danger and calamity from fire that now exiſts. The diſtance of the reſervoir from the fire is of no conſequence, provided the pipes were laid with ſufficient water way; and it would ſeldom happen that more than one fire at a time would break out in the ſame pariſh, or if it did, the ſame ſpeedy and effectual aſſiſtance could be given by any number of ſuch powerful and ſelf-working engines, as long as there was water in the reſervoir. And even ſuppoſing the ſupply from the reſervoir to be limited to any given time of expenditure, common prudence would lead to its being repleniſhed by the uſual mode, before it could be entirely exhauſted.

The

The drawing Fig. 2, ſhows how the ſyphon engine is to be applied to ſtreams of water, the advantages of which are, that the engine as well as the mill work, or manufacturing machinery it may drive, may be placed where moſt convenient, above or below the head or ſtream, to be worked by a fall of water from the leaſt to the greateſt height, or by any ſtream or river, the tail water below acting and having as much power as the head, anſwering to the height of either. Nor can a drop of water eſcape without performing its full duty. The power is therefore greater, and not liable to the diſadvantages attending a water wheel. In tide waters it would work ebb and flood ſo long as there was a difference of two feet or leſs in the height, regulating itſelf, ſo that the power may be equal, let the head and tail water riſe or fall, by which a ſmooth uniform motion is maintained and adapted to the ſmalleſt as well as the ſtrongeſt power wanted. If the tail water re-acts upon a water wheel, it muſt loſe ſo much of the power of the fall, or in other words, whenever the tail water riſes above the loweſt waſh boards of the wheel, a counter action will be exerted as is well known againſt the power of the wheel. A conſiderable quantity of the ſtream muſt likewiſe eſcape without any good effect. Theſe diſadvantages of the water wheel, are removed by the ſyphon engine; and in froſty weather being fed by a pipe from below the ice, it will not be impeded, becauſe its velocity in paſſing through the engine, will prevent the water being expoſed a ſufficient time to the cold atmoſphere to congeal it; and when at reſt the engine may be left empty. It is ſcarce neceſſary to obſerve, that when the engine is fixed below the running head of the ſtream, it will fill as ſoon as the ſluice is opened, and ſet itſelf to work; but when placed above the head, it will require ſufficient water to fill the whole interior ſpace, which being thus charged and converted into a ſyphon, will then work as well and with as much power as if the engine were placed below the head of the ſtream.

Other uſes and applications of the engine.

Froſty weather affects it leſs than mills and other hydraulic machines.

I am,

Dear SIR,

your moſt obedient and

very humble ſervant,

JOHN HARRIOTT.

Thames Police, Dec. 20, 1802.

REMARKS.

REMARKS.—W. N.

Confideration of the two principal objections to a water engine inftead of a crane.

The convenience and facilities afforded by Mr. Harriott's Engine, will be obvious to every one; and the only circumftances which can be urged in the way of objection againft it, are thofe which he has himfelf noticed; namely, that it in general requires the water to be raifed, which is to work it as a crane, and that this water is fubject to freeze. To meet thefe difficulties fairly, we muft admit, that if a ton of water were raifed to the top of a warehoufe, in order by its fall to raife a ton of goods (even if it could do that) it would be fimpler for the warehoufeman to raife his goods without the intervention of the fluid. But while we admit this general truth, it muft alfo be obferved, that the pofitions do not include the whole of the actual cafe. The raifing and lowering of goods being a procefs of confiderable fkill and intelligence, and being neceffarily carried on with many ftops, interruptions, and variations of force, is on thefe accounts performed by the moft expenfive of all firft movers; namely, the ftrength of men, and even this power is for a large part of the time inactive; namely, during all the intervals of operation. Here then is a wide field for the faving of force, if it could be ftored up and ufed when wanted. Suppofe for example, that two men were employed in a crane, and that the paufes of inactivity amounted to one third of their whole time, thefe men would be more beneficially employed in raifing water, to be afterwards directed by a boy, or by the foot of a clerk who ftood by, to keep account of the delivery of packages. But as horfe work is reckoned at leaft five times as cheap as human labour, the faving of labour by employing that animal, would be about fix fevenths of the whole; and fteam engine work upon a very fmall fcale indeed, would be as cheap again as horfe work. Hence it appears, that after every allowance for the greater quantity of water required to produce velocity by its fall, and for other circumftances the faving of labour muft be very great, by thus œconomizing and ftoring up the force, exclufive of the conveniences detailed by Mr. Harriott in his letter.

Labour of raifing the Water, compared with that now employed to hoift and lower goods. Men. Horfes. Steam. The labour faved by Mr. Harriott's engine (fupplied by fteam) would exceed fix fevenths of that now expended.

Froft would moft probably affect it in mill work as it does other engines;

The freezing of the water is an impediment of fuch a nature, that it will perhaps be found that the operations of natural

tural ſtreams or falls of water would be ſubject to interruptions from this cauſe, with his engine about as ſoon as with others that are more expoſed, ſo that its advantages would conſiſt of the other particulars which he has detailed. But with the engine applied for raiſing goods, it is to be apprehended that the water in the elevated reſervoir would be bound up by freezing, and alſo in ſevere weather checked if not confined by the ſame cauſe in the engine itſelf. The remedy for this appears to be to work the engine, by raiſing the ſame maſs of fluid repeatedly, and diſſolving ſome cheap material in it which ſhould render it leſs diſpoſed to freeze. Moſt ſaline bodies would have this effect, and the quantity once added, would remain long without waſte or loſs. Experiment would ſhew what ſalt, and whether earthy, neutral, metallic, alkaline, or acid, might be the cheapeſt, moſt effectual, and leaſt diſpoſed to act upon the engine *.

but in crane-work the water may be rendered incapable of freezing in our climate;

by a moderate addition of ſome cheap ſaline ſubſtance.

IX.

On the Electricity of the Shavings of Wood. *By Mr.* W. Wilson.

SIR, *London, Dec.* 13, 1802.

YOUR readineſs to inſert my letter of the 11th of October, encourages me to requeſt the inſertion of the following account of experiments on the electricity obtained by cutting of wood, &c. In doing which you will very much oblige

your obedient humble ſervant,

WM. WILSON.

HAVING frequent occaſions to work very dry wood that has lain over a large fire for ſeveral hours, I have often obſerved the ſhavings, &c. to adhere to the tools, and whatever they touched. About two years ago I began to take particular notice of, and endeavour to find the cauſe of this phe-

Wood ſhavings adhere to the tools.

* In the conſideration of this engine, the attention of the reader will be directed to the preſſure engine of Mr. Trevithick, in our firſt volume.

 nomenon,

nomenon, and after satisfying myself that it was not occasioned by any moisture or roughness on the substance; I suspected that electricity was the cause, and I accordingly set about the experiments which are the subject of this communication.

Dry warm beach shaved with glass is positive,

I laid a circular tin plate, 6 inches in diameter, on the cap of Bennet's electrometer, and with a piece of dry and warm window glass, scraped a piece of dry and warm beach, a few of the shavings being received on the tin plate, made the gold leaf strike the sides of the bottle with positive electricity. This was always the case whether the wood was hot or cold, but not always equally strong.

with steel negative.

I thought a knife would be more convenient than glass to scrape with, and when I tried it, I found the shavings were negatively electrified, although they were taken from the same piece of wood which before gave positive.

Other woods.

This change in the result induced me to try different woods in different ways (sometimes scraping and sometimes cutting small chips) but obtained very uncertain results, for sometimes I obtained positive and sometimes negative electricity, even when I cut the same piece of wood with the same knife.

An insulated knife acquires the contrary electricity.

I next fixed the blade of a penknife into a glass tube, covered with sealing wax, and set to work with this insulated knife, and found that it was always electrified with the contrary electricity to that of the chips which were most frequently positive. But as they were sometimes negative, I repeated the experiments very often to discover the cause, but with very little satisfaction.

A sharp insulated knife gave negative chips; a blunter positive.

However, after making several hundred trials, I found the keenness of the edge of the knife had some influence, for one day after chipping with the insulated knife, and getting positive chips, I set the knife on a hone to make it cut better (which I had frequently done before) and when I began to chip the same piece of wood which but just before gave positive chips, I found the chips were negatively electrified several times, I then chipped the same piece of wood with a knife that had been very much used, without sharping, and this gave positive chips as the other had done before it was sharped; the knife that had been sharped was tried again, but the chips were positive now, however when it was sharped again it gave negative chips.

I now

I now thought I had found the caufe of the contradictory refults, but to be more certain about it, I began the following fet of experiments. I fharpened a penknife to a very nice edge, and ufed the fame pieces of wood that had been ufed in the former trials. In 24 trials with cherry-tree, the chips were always negative, and in four trials with elm, and in fix with yew, the chips were always negative. I now drew the edge of the knife lightly over a piece of iron to dull it, expecting to get pofitive chips, but on trial the chips were ftill negative. Suppofing the knife was not dull enough, I drew the edge over the iron again, and made the edge very bad, but the chips were ftill negative. The edge of the knife was next made rough by rubbing it on a grindftone, and this rough edge gave negative chips. The knife was next ground and fet on the hone very carefully, and this fharp edge gave negative chips.

Other contradictory experiments.

As nothing fatisfactory was obtained from this fet of experiments. I began to fufpect that the degree of heat of the wood was to be confidered (for the wood was cold in all the laft experiments, and fometimes hot, and fometimes cold, but moft frequently hot in the firft) or that perhaps the heat of the wood, and the fharpnefs of the knife, were both to be taken into the confideration. I therefore fet about the following fet of experiments.

Probability that the heat of the wood might influence the refults.

I fplit the piece of cherry tree (that was ufed before) into two pieces, one of which was made thoroughly hot at the fire. This when chipped with the fame knife that was ufed in the laft experiments, without being fharpened, gave pofitive chips every time in fix trials, and after this piece of wood had cooled till it was fcarcely warm, gave pofitive chips every time in four trials. I then took the other piece which had not been near the fire for five or fix hours, this gave negative chips every time in four trials. I now made this piece of wood quite hot, thoroughly, and chipped it again with the fame knife, and in feven trials the chips were pofitive every time. Thefe two pieces of wood was now laid by for three or four hours to get quite cool. In this ftate they gave negative chips every time in twelve trials. One of them when made thoroughly hot again, gave pofitive chips every time in fix trials. The other piece was now made warm (but only externally fo) in eight trials it gave pofitive chips four times,

Detail of experiments with hot and cold wood; and different fharpnefs of the knife.

and negative chips four times; but after laying three or four hours to cool it gave negative chips every time in eight trials. A third piece of cherry-tree that had not been near a fire for four or five days gave negative chips. I repeated these experiments with different knives that had not very sharp edges, and with beach as well as cherry-tree. And whenever the wood was made thoroughly hot at the fire, it always gave positive chips, not only when hot, but when it was so cooled as not to be sensibly warm; but when it had laid away from the fire three or four hours, the chips were always negative. Sometimes when the wood was but slightly warmed, it would be very difficult to get any signs of electricity, and at other times when the wood was made hot only externally (by putting it very near the fire for a short time) the first few chips would be positive, and the succeeding ones negative. I had one instance where with the first chip the electrometer diverged near an inch, and with the second it completely closed again. Having succeeded thus far, I thought I would try whether the results would be the same if I used a very sharp knife, and accordingly sharped two knives on a hone very carefully. And I used the same pieces of cherry-tree made thoroughly hot; in nine trials with one of the knives, the chips were negative every time and in five trials with the other knife, the chips were negative every time. I made a number of similar trials with a piece of beach with always the same results, but the beach seemed to be too hard for that keenness which is necessary to produce negative chips, for after cutting one or two chips, the edge would be spoiled, and produce positive chips; but always when the knife was sharped, the first one or two chips would be negative. Similar to this I found by subsequent trials to be the case with the pieces of cherry-tree, ten or twelve chips of this (which was very straight and open grained) would spoil the edge.

General results. Shavings of dry wood by glass, are negative—hot wood by a moderate steel edge is positive, but negative if cold:—and a very sharp edge gives negative whether hot or cold.

From these experiments it appears, that when very dry wood is scraped with a piece of window glass, the shavings are always positively electrified. And if chipped with a knife, the chips are positively electrified if the wood is hot, the edge of the knife not very sharp, and negatively electrified if the wood is quite cold. But if the edge of the knife is very keen, the chips will be negatively electrified whether the wood is hot or cold.

2 The

The greateſt number of trials was made with the inſulated knife, which was always electrified contrarily to the chips; but the ſurface of the wood where the chips were cut from was very ſeldom electrified, and when it was it was always but weakly ſo, and of the ſame denomination as that of the weakeſt of the other two. I have repeatedly found that if a piece of dry and warm wood is ſuddenly ſplit aſunder, the two ſurfaces which were contiguous are electrified, one ſide poſitive and the other negative.

The knife always contrary.

Split wood has the two ſtates.

X.

On the Compoſition of Emery. *By* SMITHSON TENNANT, *Eſq.* *F. R. S.* (*Ph. Tranſ.* 1802.)

THE ſubſtance called emery, which, from its great hardneſs, has been long uſed in various manufactures, for grinding and poliſhing other bodies, has not, it appears, been hitherto correctly analyzed. In books of mineralogy, it is conſidered as an ore of iron; an opinion probably derived from its great ſpecific gravity, as well as from the iron which it frequently contains. But, where this metal is moſt abundant, it could not be extracted from it with advantage, and ought rather to be regarded as an impurity, as it does not contribute to produce the peculiar hardneſs for which this ſubſtance is diſtinguiſhed. In Mr. Kirwan's mineralogy, he mentions an examination of emery made by Mr. Wiegleb, from which he inferred that 100 parts conſiſted of 95,6 of ſilex, and 4,4 of iron. Mr. Kirwan, however, juſtly ſuſpects the correctneſs of this account, and obſerves that he had no doubt but ſome other ſtone was impoſed on Mr. Wiegleb for emery.

Emery has not yet been correctly analyzed.

When powder of emery is boiled in acids, it becomes of a lighter colour, from the loſs of part of the iron; after which, it does not ſeem to undergo any further alteration. As acids produce ſo little effect on it, I expoſed it to a pretty ſtrong red heat, with carbonate of ſoda, in a crucible of platina. On adding water to the maſs contained in the crucible, the greater part of the emery was found in powder; having only become of a light colour, from the extraction of part of the iron.

Emery powder gives part of its iron to acids.

Soda by fuſion diſſolved ſome argil and leaves the emery.

iron. Though this procefs was twice repeated with the remaining powder, and in a ftronger heat, a great porportion of it remained undiffolved.

The alkaline folution, after a red calx of iron had fubfided from it, was faturated with acid; and gave a precipitate of a white earth, which I found to be almoft purely argillaceous.

This refult is fimilar to Klaproth's with diamond fpar.

The refult of thefe experiments, was fo fimilar to thofe of Mr. Klaproth on diamond fpar, as to render it very probable that emery was in reality the fame fubftance, though ufually mixed with a larger proportion of iron; and the fubfequent experiments appear to confirm this opinion.

Emery pulverized and cleared of magnetic particles.

In order to obtain a quantity of emery as free from iron as I could, I reduced to a coarfe powder, a piece which confifted of different ftrata, fome of which were of much lighter colour than others; and afterwards feparated, by a magnet, the particles which were attracted by it. The part which was not attracted by the magnet, I obferved to have the ufual degree of hardnefs (by the fcratches which might be made with it on flint.) I then reduced it to a finer powder, in an agate mortar; and, as this was principally done by preffure, and not by grinding, hardly any fenfible addition was made to its weight. In the fame manner, I found that diamond fpar might be powdered to the fame degree of finenefs, without any material increafe of weight from the mortar.

20 of the clear powder fufed with 120 foda,

Of the emery powder thus prepared, 20 grains were taken, and heated in the manner before defcribed, with 120 grains of foda, which had been previoufly deprived of carbonic acid, and boiled to drynefs in a filver pan. By nearly the fame procefs as that ufed by Mr. Klaproth, I obtained about 16,0 grains of argillaceous earth, ,6 of filiceous earth, ,8 or ,9 of iron, and ,6 of a grain remained undiffolved. Thefe numbers, reduced to parts of a hundred, are therefore,

and by the fame procefs as Klaproth's the component parts of emery

Argillaceous earth - - - -	80
Silex - - - - - -	3
Iron - - - - - -	4
Undiffolved - - - - -	3
	90.

Mr.

Mr. Klaproth obtained from the Chinese corundum, after separating from it the particles which were attracted by the magnet,

proved nearly the same as those of Chinese corundum.

Argillaceous earth - - -	84
Silex - - - -	6,5
Iron - - - - -	7,5
	98.

As this analysis was no doubt conducted with greater care than mine, the loss of weight was less; but the proportion of the ingredients is sufficiently near to show that the substances are essentially the same.

Other emery containing one third part iron.

From 25 grains of emery which appeared the most impregnated with iron, and yet retained its usual hardness, I obtained, argillaceous earth 12,5, silex 2, iron 8, and one grain was not dissolved; or, per cent.

Argillaceous earth - - -	50
Silex - - - - -	8
Iron - - - -	32
Undissolved - - - -	4
	94.

As such emery can easily be had of uniform quality in large pieces, I procured the powder employed in this experiment, by rubbing two pieces against each other.

Another analysis.

From 25 grains of emery, similar in appearance to the preceding, but which had been digested with marine acid previous to the action of the alkali, I had,

		per cent.
Of argillaceous earth - -	16,4	65,6
Siliceous earth - - -	,8	3,2
Iron - - - - -	2,	8,
Not dissolved - - - -	4,5	18,0
	23,7	94,8.

Emery appears as hard as diamond spar.

The hardness of emery, as far as I could judge by its cutting rock crystal and flint, appeared to be equal to that of diamond spar. The latter could not be scratched by the former; but, as emery has not a surface sufficiently polished to render a mark visible, the converse of this could not be tried.

All

Emery comes chiefly from Naxos.

All the emery which is uſed in England, is ſaid to be brought from the Iſlands of the Archipelago, and principally from Naxos. In thoſe places, it is probably very abundant; as the price of it in London, which I was told was 8 or 10 ſhillings the hundred weight, appears little more than ſufficient for the charges of carriage. Though I ſaw a very large quantity in one place, (more than a thouſand hundred weight,) I could not find any pieces of a cryſtallized form; poſſibly the great proportion of iron uſually mixed with it, may prevent its cryſtallization. The whole conſiſted of angular blocks incruſted with iron ore, ſometimes of an octaedral form, with pyrites, and very often with mica. The latter frequently penetrates the whole ſubſtance of the maſs, giving it, when broken, a ſilvery appearance, if ſeen in the direction in which the flat ſurfaces preſent themſelves to the eye. As theſe ſubſtances have no chemical relation to the emery itſelf, it is remarkable that they ſhould alſo accompany the diamond ſpar from China; for Mr. Klaproth obſerves, "that its lateral "facets are moſtly coated with a firmly-adhering cruſt of "micaceous ſcales, of a ſilvery luſtre:" he alſo mentions, beſides felſpar, pyrites, and grains of magnetic iron ore.

It is cheap; not cryſtallized; is accompanied by the ſame ſubſtances as Chineſe diamond ſpar.

XI.

Experiments and Obſervations on the Power of Fluids to conduct Heat; with Reference to Count Rumford's Seventh Eſſay on the ſame Subject. By JOHN DALTON *.

The properties of heat are continually under our notice.

THE nature and properties of fire or heat are ſubjects which preſent themſelves to our conſideration in almoſt every department of phyſics: it is no wonder therefore that new experiments, which point out and define the modes of operation of fire, before unobſerved, or at leaſt too much overlooked, ſhould attract the attention of philoſophers.—Theſe obſervations were ſuggeſted upon reading Count Rumford's very ingenious experiments, in his eſſay above-mentioned, which exhibit a fact in a more ſtriking point of view than it has appeared

Count Rumford's experiments on the tranſmiſſion of heat by circulation through fluids.

* Manchester Memoirs, vol. v. 373.

peared before—namely, *that the quickneſs of the circulation and diffuſion of heat in fluids, is occaſioned principally by the internal motion ariſing from a change of ſpecific gravity affected by the heat.*—But the concluſion he has drawn from them—*that fluids are perfect non-conductors of heat,* in the way in which ſolids conduct it, appears to me totally unwarranted from the experiments, and erroneous in itſelf. And as it may be an error of practical conſequence, if adopted, the expoſition of it ſeemed deſirable—which is the object of the following remarks and experiments.

His inference, that fluids are non-conductors, erroneous.

My firſt attempt was to aſcertain the preciſe degree of cold at which water ceaſes to be further condenſed—and likewiſe how much it expands in cooling below that degree to the temperature of freezing, or 32°. For this purpoſe I took a thermometer tube, ſuch as would have given a ſcale of 10 inches with mercury from 32° to 212°, and filled it with pure water. I then graduated it by an accurate mercurial thermometer, putting them together in a baſon filled with water of various degrees of heat, and ſtirring it occaſionally: as it is well known, that water does not expand in proportion to its heat, it does not therefore afford a thermometric ſcale of equal parts, like quickſilver.

Experiments.

A thermometer made with water.

From repeated trials agreeing in the reſult, I find, that the water thermometer is at the loweſt point of the ſcale it is capable of, that is, water is of the greateſt denſity at $42\frac{1}{2}°$ of the mercurial thermometer. From 41° to 44° incluſively the variation is ſo ſmall as to be juſt perceptible on the ſcale; but above or below thoſe degrees, the expanſion has an increaſing ratio, and at 32° it amounts to $\frac{1}{8}$th of an inch, or about $\frac{1}{100}$th part of the whole expanſion from $42°\frac{1}{2}$ to 212° or boiling heat.—During the inveſtigation of this ſubject, my attention was arreſted by the circumſtance, that the expanſion of water was the ſame for any number of degrees from the point of greateſt condenſation, no matter whether above or below it: thus I found that 32°, which are $10°\frac{1}{2}$ below the point of greateſt denſity, agreed exactly with 53°, which are $10°\frac{1}{2}$ above the ſaid point; and ſo did all the intermediate degrees on both ſides. Conſequently when the water thermometer ſtood at 53°, it was impoſſible to ſay, without a knowledge of other circumſtances, whether its temperature was really 53°, or 32°. Recollecting ſome experiments of Dr.

Greateſt contraction at $42°\frac{1}{2}$.

Law of expanſion.

It is the ſame on each ſide above and below $42°\frac{1}{2}$.

and the water may be cooled down as low as 5° or 6° without freezing, &c. &c.

Dr. Blagden in the Philoſophical Tranſactions, from which it appears that water was cooled down to 21° or 22° without freezing, I was curious to ſee how far this law of expanſion would continue below the freezing point, previouſly to the congelation of the water, and therefore ventured to put the water thermometer into a mixture of ſnow and ſalt, about 25° below the freezing point, expecting the bulb to be burſt when the ſudden congelation took place. After taking it out of a mixture of ſnow and water, where it ſtood at 32° (that is 53° per ſcale) I immerſed it into the cold mixture, when it roſe, at firſt ſlowly, but increaſing in velocity, it paſſed 60°, 70°, and was going up towards 80°, when I took it out to ſee if there was any ice in the bulb, but it remained perfectly tranſparent: I immerſed it again, and raiſed it 75° per ſcale, when in an inſtant it darted up to 128°, and that moment taking it out, the bulb appeared white and opake, the water within being frozen: fortunately it was not burſt; and the liquid which was raiſed thus to the top of the ſcale was not thrown out, though the tube was unſealed. Upon applying the hand, the ice was melted, and the liquid reſumed its ſtation. This experiment was repeated and varied, at the expence of ſeveral thermometer bulbs, and it appeared that water may be cooled down in ſuch circumſtances, not only to 21°, but 5° or 6°, without freezing, and that the law of expanſion abovementioned obtains in every part of the ſcale from 42°½ to 10° or below; ſo that the denſity of water at 10° is equal to the denſity at 75°. But as the diſcovery of this curious, and I believe hitherto unnoticed property, has little to do with the object before us, I ſhall ſay no more of it at preſent.

(To be continued in our next.)

SCIENTIFIC

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Lectureship on Subjects of Natural and Experimental Philosophy, at Newcastle upon Tyne.

AMONG our provincial societies for promoting the cultivation of literature and philosophical pursuits, the society at Newcastle upon Tyne, has for a number of years possessed an eminent station for the ability of its members, and the unremitting assiduity and intelligence which they have exerted to discover and carry into effect those objects which bodies of men are best capable of promoting. We all know that it is easy for a body of men to assemble together; to dignify themselves by a name; to hold forth striking pretensions to the uninstructed multitude; and to make a sort of commercial traffic of that scientific celebrity which the voices of men can confer upon each other, for a time at least, while the great public may ask in vain what they have done to deserve it. But when a body of true friends to their country and to mankind meet together, to consider in what way they can best employ their talents for the good of society and the promotion of the arts of civilized life, within that district which more immediately falls under the limit of their notice, and the extent of their power and influence;—when their well-earned celebrity is a secondary object, in comparison with the internal satisfaction afforded by a proper employ of talents and virtue—such an association becomes a public blessing, and its good consequences extend to the remotest periods of after times. The Literary and Philosophical Society of Newcastle, has already made inquiries into the state of the arts, of agriculture, and of the mineral products of their vicinity. It has assisted the diffusion of knowledge by the establishent of a well regulated and easily accessible library; and it has lately proceeded to constitute a lectureship on the subjects of natural and experimental philosophy, the requisite steps for which are now in progress with great spirit and activity.

Establishment of a philosophic lecture at Newcastle upon Tyne.

Active exertions of the society at Newcastle.

On the fourth of May last, a paper by Mr. Thomas Bigge, on the expediency of establishing a lectureship at Newcastle upon Tyne, upon the subjects of natural and experimental philosophy, was read: In this paper, we find a clear, elegant and

History of the proposed lecture.

Mr. Bigge's proposal

and animated ſtatement of its objects; conſidered as well with regard to their great influence on ſociety, as to thoſe local circumſtances towards which the intention of the author was more particularly directed. In this paper, no leſs ſtriking for the value of its contents, than for the eſtimable motives of its author, we find facts which ſtrongly ſupport what has been ſaid at the commencement of this article concerning the general proceedings of this reſpectable ſociety. In conſequence of this ſuggeſtion, the ſubject was again reſumed, and an addreſs to the public was circulated in the month of June, requeſting their co-operation and aſſiſtance, exhibiting ſome part of the outline, and announcing that the appointment of Lecturer has been made to the Rev. Wm. Turner, a gentleman long and well known to the ſociety for the abilities with which he had exerciſed the office of ſenior ſecretary. Since that time, conſiderable progreſs has been made in the ſubſcriptions; and the lecturer delivered a general introductory diſcourſe on Tueſday, November the 16th laſt, upon the objects, the advantages, and the intended plan of the lectures. I am ſorry that the limits of a ſhort ntoice forbid the attempt to give any analyſis of this excellent diſcourſe, which affords ample evidence of the comprehenſive views of the author, with regard to the organization and duties of ſociety in general, as well as the ſciences he has undertaken to teach. I have thought it might be advantageous in ſome degree to the views of this public body to give the preſent account, ſhort and imperfect as it muſt neceſſarily be, and ſtill more to the world at large, if the exertions in favour of the ſciences in one part of the kingdom, ſhould, as is moſt probable, be followed by ſimilar proceedings in others.

adopted, and an addreſs to the public circulated.

On a new Kind of Mortar called Plaiſter Cement.

Account of a water cement.

Among the ſtones on the ſea-beach, near Boulogne, a particular kind is collected, which when calcined and pounded like plaiſter, forms a very hard cement with water. This ſubſtance has been uſed for œconomical purpoſes, and was found to poſſeſs the valuable quality of reſiſting water; under which fluid, it hardens very ſtrongly, and much more than in the

the air. Many conſtructions made with this cement afford the moſt complete proof of its ſolidity and tenacity. The detail of theſe experiments are given in a report preſented by *Le Sage* to the ſociety of Agriculture, Commerce and Arts, at Boulogne. Theſe muſt no doubt be of much local importance; but the analyſis of Guyton, as it tends to elucidate the compoſition of cements, is of more extended conſequence.

The ſpecific gravity was between 2,04 to 2,19 and 10 grammes, or about $\frac{1}{3}$ of an ounce produced in centegrammes.

Lime - - - -	403
Carbonic acid - - -	330
Clay - - - -	187
Oxide of iron - - -	70
Alumine - - - -	5
	995

The 187 centegrammes of clay afforded,

Silica - - - -	99
Alumine - - - -	39
Oxide of iron - - -	43
	181

The ſtones are therefore compoſed of

Lime - - - -	403
Carbonic acid - - -	330
Oxide of iron - - -	113
Silica - - - -	99
Alumine - - - -	44
Loſs - - - -	11
	1000

Citizen Guyton ſhewed a veſſel to the ſociety d'Encouragement at Paris, which was very cloſe and firm in its texture, whence the abridger in the bulletin des Sciences, from which I take this article, expreſſes his opinion that it would prove very uſeful in the fabrication of various articles of pottery.

Not

Note concerning Two Brothers of a Race of Men resembling Porcupines.

Men having scales and spines like porcupines.

Many philosophers have already spoken concerning this race, the reality of which has been established in a family well known by the name of Lambert. Two brothers of this family, all the males of which, have their bodies covered with spines or scales, are at present in Paris. One is 24 and the other 14 years of age. The body of the eldest is entirely covered, except the head and the inside of the hands and feet: the youngest is naked in some places, particularly about the breast; but the brown spots on those parts sufficiently indicate that when he advances in age, he will become as rough as his brother. The spines on the back of the hand are very large, and may be compared for their diameter to the quills of the porcupine; but those upon the breast have a greater resemblance to scales: they are small long plates, very numerous, and near together, being vertically implanted in the skin.

This thickening of the epidermis and hair, proceeds from a disease transmitted from generation to generation, but only from male to male. Five generations have been already afflicted with it.

Bull. de la Soc. Philomath.

Mawe's Mineralogy of Derbyshire.

The Mineralogy of Derbyshire, with a Description of the most interesting Mines in the North of England, in Scotland, and in Wales; and an Analysis of Mr. Williams's Work, entitled, "The Mineral Kingdom." Subjoined is a Glossary of the Terms and Phrases used by Miners in Derbyshire. By JOHN MAWE, *London. Phillips, George Yard,* 1802, *octavo,* 211 *Pages, with* 4 *Engravings.*

The author of this useful book was employed by a Spanish gentleman to make surveys of our principal mines in Derbyshire, to collect their various productions, and more particularly specimens from each stratum, describing their thickness, situation, and position; in order to shew an exact representation of the mines for the royal cabinet at Madrid. To the

 observations

observations thus made, he has added accounts of some mines in Scotland; a Tour from Glasgow to Staffa; the salt mines at Northwich; the Parys Mine; Observations in Wales, and an Account of Mr. Williams's book, in 2 vols. called the Mineral Kingdom. This account seems too concise to be of great utility, and as a critique would have answered its purpose as well, if a few of the epithets had been more civil.

An Enquiry into the Causes of the Errors and Irregularities which take place in ascertaining the Strength of Spirituous Liquors, by the Hydrometer, with a Demonstration of the Practicability of simplifying and rendering this Instrument accurate. By WILLIAM SPEER, *Supervisor and Assayer of Spirits in the Port of Dublin. Octavo, 48 Pages. London,* 1802.

Speer on the hydrometer.

On the present occasion, when the government of the country appears disposed to settle this point of experimental philosophy, upon which results of considerable importance to the revenue and to private property depend, but which have certainly been hitherto conducted in a slovenly and inaccurate manner, it must be very acceptable to all parties to see the subject clearly treated in a small pamphlet. After a short Introduction, the author gives an account of the origin and rise of the hydrometer, the causes of its irregularities, the different modes of charging overproof; the principal defects in Clarke's hydrometer; the imperfection of the weights in that instrument; the general principles of floating instruments; the standard of pure spirit; Gilpin's tables; modes of simplifying the hydrometer; description of his own improved hydrometer: with answers to objections that probably might be made to it; and he concludes with a chapter on the necessity of a standard for proof, and other practical objects of consequence in this business.

M. Speer's own hydrometer consists of a ball and stem, with counterpoise beneath as usual, and the upper or graduated stem is made of an octagonal form. Upon each of the eight faces is engraved a scale of per-centages; by inspection of which, the quality of the spirit is seen. But as the instrument will sink to different depths in the same spirit, according to its

Description of Speer's hydrometer.—Stem octagonal; each side having the *per-centages* for different temperature.

Speer on the Hydrometer.

its temperature, the ſcale upon each of the faces is adapted to a determinate temperature, namely 35°, 40°, 45°, &c. till the laſt, which is for 70°. The temperature of the ſpirit being therefore known, the reſult muſt be read, upon that face, at the top of the which the known temperature is engraved; and to prevent any miſtake, there is a ſmall index to be put on the ſtem to direct the eye of the obſerver to the proper face. And moreover, as the temperature is ſhewn only to every five degrees, there is another index of a different colour, which performs the office of a weight, and ſhews the intermediate temperature. This may alſo be effected by warming the ſpirits, by holding the glaſs in the hand till its temperature agrees with that marked on one of the faces; or, as I underſtand from the ſpecification of the author's patent, the preciſion of a ſingle degree, if required, may alſo be obtained, by four ſmall pins to be inſerted, one for each intermediate degree in holes, in the counterpoiſe below, where in fact they operate as weights of adjuſtment.

On the laſt day of this month, Mr. MACARTNEY will commence a COURSE OF LECTURES *in the* MEDICAL THEATRE OF SAINT BARTHOLOMEW'S HOSPITAL, *on the Anatomy of Animals and Vegetables,* from which will be deduced the general Doctrines of Phyſiology, or the laws of Organized Matter.

For Particulars, apply to Mr. NICHOLSON, at the *Apothecary's Shop, St. Bartholomew's Hoſpital.*

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY, 1803.

ARTICLE I.

Answer to the Observations of Mr. WILLIAM CRUICKSHANK *upon the Doctrine of Phlogiston* *. *In a Letter from the Rev.* JOSEPH PRIESTLEY, *L. L. D. F. R. S. &c.*

To Mr. NICHOLSON.

Northumberland, America, Nov. 5, 1802.

SIR,

YESTERDAY only I received the fifth Number of your valuable Journal, viz. for May, which contains Mr. Cruickshank's reply to my defence of the doctrine of phlogiston in answer to him; and it gives me sincere pleasure to observe, that he shews so much zeal in the defence of his hypothesis, as by this means the truth will appear the sooner. I only lament, that my great distance will necessarily make the controversy of much longer continuance than it would otherwise be. **Introductory paragraph.**

Mr. Cruickshank censures me for misquoting his words, and mistaking his meaning. If I have done so, I can assure him that it was without design; nor do I perceive that what he alledges **The quotation discussed by Mr. Cruickshank stated to be of no consequence to the argument.**

* Philosophical Journal, II. 48.

alledges of this kind is of any confequence to the main argument. On the other hand, he fays, p. 42, with an air of triumph, but before the victory, that "having additional difficulties to ftruggle with, I have been under the neceffity of adopting new, and fometimes contradictory, opinions, in my explanations and defences."

The difcuffion attended with no difficulty.

Now I really am not fenfible of any difficulty whatever in this difcuffion; and in advancing this he feems not to have apprehended my fentiments with refpect to the calces of metals. If he was acquainted with my publications, he would have found that there was nothing either new, or contradictory, in what I have now advanced on this fubject, or that of the compofition of charcoal; though that I have fometimes changed my opinions on philofophical as well as other fubjects, I readily acknowledge, and without any feeling of fhame.

Finery cinder contains water only. For (upon adding phlogifton) water only is feparated.

In my opinion, and that of long ftanding, the black calx of iron, commonly called *finery cinder*, contains no oxigen, but only water; and though the calx of zinc, and the yellow calx of lead, called *mafficot*, do contain fome, it is fo little that I cannot detect it. Mr. Cruickfhank fays, p. 42, that "if water be the only fubftance contained in oxides, heat alone ought at leaft to revive fome of them, and that in this cafe nothing but water would be feparated." Now what I maintain is, that when finery cinder is revived, (which, however, is not done without the introduction of phlogifton) nothing but water is feparated from it.

The prefent letter is confined to that object.

Oxigen is undoubtedly contained in the red calx of lead, called *minium*, and in that of mercury; but I fay that even thefe cannot be revived without the introduction of phlogifton. This fubject, however, I wave for the prefent, wifhing to difpatch that of the air from finery cinder and charcoal before we proceed to any other; and I wifh Mr. C. to attend to the following obfervations, which I fhall now ftate more diftinctly than I have done before, that it may be the eafier for our readers to judge between us.

Mr. C's theory demands that oxigen fhould quit iron to form fixed air with charcoal, and then return to the iron in order

1. Mr. Cruickfhank's hypothefis requires that, in the procefs of heating finery cinder and charcoal, the oxigen in the finery cinder fhould quit that fubftance, and unite with carbon in the charcoal, in order to form fixed air. Since, however, this fixed air is to be decompofed by iron, the oxigen which it

 has

has got from the finery cinder muſt be ſeparated from it, and enter into the ſame calx again. But while the heat continues the ſame, I deem theſe contrary effects to be impoſſible. If the degree of heat that is applied expel oxigen from the calx, it will certainly prevent its return. Conſequently, if fixed air be formed, it cannot be decompoſed in theſe circumſtances.

to leave oxide of carbon; which cannot happen at the ſame heat.

2. If it was poſſible for oxigen to be ſeparated from finery cinder without any thing entering into it, which I think cannot be done, it could not, according to the new theory, form any thing by its union with carbon beſides *fixed air;* this being ſaid to be carbon diſſolved in oxigen gas, but nothing inflammable could be produced by their union. Of this Mr. Cruickſhank is ſenſible, and therefore he was under the neceſſity of ſuppoſing that, after the oxigen had quitted the finery cinder, it muſt enter into it again. But if this were poſſible, nothing would remain of the fixed air but *carbon,* as before that union; and this is a ſolid ſubſtance, incapable, without the aid of oxigen, of aſſuming the form of air. Whence, then, comes the inflammable air in this proceſs, which ſo nearly reſembles that from charcoal and water, that they muſt have the ſame origin; and with reſpect to *this,* Mr. Lavoiſier decides, that whatever is inflammable in it muſt come from water.

Carbon being conſidered as forming only one compound with oxigen (namely fixed air) it is concluded that no inflammable air could be had in reducing finery cinder; unleſs by means of water.

3. Admitting all that Mr. Cruickſhank alledges concerning the difference in ſpecific gravity, and other circumſtances, between the air from finery cinder and charcoal, and that from water and charcoal, it is not ſo great as the difference between the latter and the light inflammable air from the metals with acids or water. Different as they may be in other reſpects, they are all inflammable, and have the common property of uniting with oxigen in a certain degree of heat; in conſequence of which they are the reverſe of *oxides,* and muſt be claſſed with *combuſtible* ſubſtances, equally with ſulphur and phoſphorus.

The inflammable gas, though different from that obtained by charcoal and water, is not an oxide but a combuſtible.

4. If the oxigen, after quitting the finery cinder, entered into it again, it would make it finery cinder as at the firſt, or at leaſt in ſome degree; whereas the calx is completely revived in this proceſs, the iron ſo revived being as ſoluble in acids as any iron whatever.

The oxigen does not return to the finery cinder; for the iron is reduced and continues ſo.

5. If the iron ſhould be completely revived by the oxigen wholly leaving it, I ſtill maintain that it could not, by any degree

Iron cannot decompoſe fixed air. Mr. C's experiment leſs

decisive than that of the author.

degree of heat decompoſe fixed air. For my experiment with a *burning lens*, in which it could not be done, is far more unexceptionable than Mr. Cruickſhank's with *bladders and a gun barrel*. His objection to my proceſs has no weight. It was made with a few ounce meaſures of the air over mercury, with a lens ſixteen inches in diameter, and continued ſeveral hours, generally from ten o'clock to one; ſo that no particle of the air could eſcape being expoſed to a far greater degree of heat than could be communicated through a gun barrel. His experiment I have frequently made both in England and here, but could never be ſatisfied with the reſult. The ſcale upon the iron, I have no doubt, came from moiſture in the air, or from the bladders. Indeed, I cannot think that any perſon converſant as I have been with both theſe modes of operating, can heſitate in deciding that the preference muſt be given to mine.

Charcoal inferred to contain oxigen from Mr. Tenant's experiments.

6. Mr. Cruickſhank ſeems to think that charcoal cannot contain any oxigen; but Mr. Tenant's fine experiment deciſively proves that it does. For where are we to look for the oxigen (which we all acknowledge to be a component part of fixed air) which is ſeparated from the marble, but in the charcoal which is produced, and in that it makes part of a ſolid ſubſtance, and does not take the form of air.

If oxigen exiſted in finery cinder it ought to produce the phenomena of combuſtion in charcoal.

7. Since oxigen and all combuſtible ſubſtances unite, and explode, in a certain degree of heat, the oxigen that is expelled from the finery cinder uniting with carbon from the charcoal when red hot muſt enable it to *burn*; and therefore in theſe circumſtances there ought to be an exploſion, or at leaſt a gradual combuſtion of them in the courſe of the proceſs, as there is when oxigen is put to the ſame ſubſtance, and heated with it afterwards.

Invitation to Mr. Cruickſhank to enter into a full diſcuſſion of the new theory.

It is now near twenty years ſince this new theory was advanced, and from that time to the preſent I have not ceaſed to expreſs my opinion of its fallacy, and to give my reaſons for that opinion; but I have not till very lately been able to draw any degree of attention to the ſubject. Now, however, I am happy to have ſucceeded in this; and as I find that the chemiſts in France, the great patrons of the ſyſtem, look to Mr. Cruickſhank as the ableſt defender of it, I earneſtly wiſh that he would undertake the diſcuſſion of every article of my objections to it. What he has animadverted upon is only *one*

out of *eleven* articles in my *Tract on Phlogiston;* and besides this, he should consider what I have advanced on the same subject in other publications, especially my experiments on the generation of air from water, both by evaporation and by freezing, and those on the pile of Volta, and also several articles in the Transactions of the Philosophical Society at Philadelphia.

As to the manner in which a controversy of this kind is conducted, whether it be expressive of *respect*, or *contempt*, it will have little weight with a judicious reader. Preferring however a plain and calm discussion,

I am,

Dear SIR,

Your's sincerely,

J. PRIESTLEY.

II.

The Construction of an Apparatus for conducting Sound and holding Conversations at a Distance. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

SIR,

SOME of the most important inventions were at first made use of, either as playthings for children, or in exhibitions to amuse the populace.

Several very important inventions were at first considered as trifling.

Printing, which has contributed more to the improvement of the human mind than any other art, was invented for amusement, and the instruction of children *. But the true value of an invention is not immediately seen, it has to go through a long series of improvements, before it arrives at a degree of perfection that is important to society.

Printing.

The loadstone was long known to jugglers, and used by them in their exhibitions, before it was applied to the important purpose of navigation: and how far the improvements in

Effects of the loadstone.

* History of Holland by Adrian Young.

mechanics,

mechanics, optics, and acouſtics, which are now exhibiting for amuſement, may hereafter become uſeful in the common affairs of men, is a matter on which we are not at preſent able to judge.

Phantaſms by the camera obſcura behind a ſcreen.

The daggers and death-heads which are made to appear in the air, by means of a concave mirror; and the ghoſts and goblins, that are conjured up by means of the magic lantern of Philipſthal, not only ſerve as amuſement, but they may contribute to check the growth of ſuperſtition, by ſhewing in an agreeable manner how eaſy it is to impoſe upon the ſenſes.

Speaking machines.

Another way of deceiving the ſenſes, is by ſpeaking machines. Theſe are of very ancient date, and have been ſo much improved by the moderns, as to attract much attention.

One by Thomas Irſon in the 17th century.

No longer ago than the reign of Charles II. one Thomas Irſon excited much wonder in the king and his whole court, by a machine of this kind *. It is now well known that the ſound was conveyed to the mouth of the ſtatue by means of tubes artfully concealed; but the principle on which the ſpeaking machines are now conſtructed, appears to be at this time very little underſtood.

Sound conducted through timber.

It was known in the days of Pliny, that if a long beam of timber received a ſlight ſtroke at one end, the ſound was diſtinctly heard by a perſon whoſe ear was applied to the other end, though it could not be heard at the ſame diſtance through the air.

This property applied to a ſpeaking apparatus.

From the following experiments which I made on this property of wood, it appears that acouſtic inſtruments may be conſtructed for converſing at a diſtance, without the aſſiſtance of tubes to convey the ſound.

EXPERIMENT I.

A deal rod 16 feet long was by contact made the medium between two trumpets.

I took a deal rod 16 feet long, and about an inch ſquare, and after having fixed one end of it into the ſmall end of a ſpeaking trumpet, I laid it upon two ſupporters or props in an horizontal poſition. One of the props was placed under the trumpet, about three inches from its wide end, and the other prop was placed near the other end of the rod. Another ſpeaking trumpet was then laid acroſs the rod, about three inches from the end. The wide end of this trumpet

* Beckmann's Hiſtory of Inventions, III. 334.

reſted

refted upon the rod, but the other end was fufpended by a ribband, from an object which need not be defcribed.

Sound was conveyed very diftinctly through this apparatus.

After the apparatus had been prepared as above mentioned, I introduced my watch into the end trumpet, and on applying my ear to the crofs trumpet, I heard the beats much louder than if the watch had been at the diftance of a few inches only. The found appeared to come out of the crofs trumpet, although the watch was at the diftance of feventeen feet and a half: and when the watch was laid into the crofs trumpet, it was heard equally well at the end trumpet.

It may not be improper to mention, that the found was increafed by introducing a piece of metal, between the crofs trumpet and the rod upon which it refted; but the manner of producing the beft effect, I found by trial.

EXPERIMENT 2.

Bodies in contact with the rod did not prevent the effect.

I placed another prop under the middle of the rod, and laid feveral books upon it in different places; but the rod conveyed the found juft as well as before.

EXPERIMENT 3.

Converfation held by found paffing through the rod both ways in a whifper.

My affiftant in thefe experiments being feated at the end trumpet, and myfelf at the other, a converfation took place through this apparatus; but in whifpers too low to be heard through the air at that diftance. The converfation afforded us much entertainment; for when the ear was placed in a certain pofition, the words were heard as if they had been fpoken by an invifible being within the trumpet. And what rendered the deception ftill more pleafing, the words were more diftinct, fofter, and more mufical, than if they had been fpoken through the air.

SPEAKING MACHINES.

Ufeful purpofes to which thefe machines may be applied.

What I have to fay on this fubject, muft be fo far anticipated by thefe experiments, as to render any particular defcription of mine unneceffary. It may not, however, be improper to mention my conjectures, refpecting fome ufeful purpofes to which this acouftic inftrument may be applied. If a communication founded on thefe principles, were made between a fhop and the dining room, or warehoufe, it might contribute to the difpatch of bufinefs: and inftruments made

on

on the ſame principles might, perhaps, be found convenient in private houſes, if introduced between the parlour and ſome other room appropriated to the uſe of domeſtics. Directions might then be given to a ſervant without his entering the room, and in whiſpers too low to diſturb the company.

I am,

SIR,

your's reſpectfully,

EZEKIEL WALKER.

Lynn Regis, Jan. 19, 1803.

ERRATA.

Vol. IV. p. 40. in the firſt col. of the table, *for* 1, 2, 2, 4, *read* 1, 2, 3, 4.
P. 43. line 12, *for* long *read* large.
P. 43. line 19, *for*, In this manner, *read* Thus.

III.

Obſervations in Reply to Mr. GOUGH's *Letter on the Grave Harmonics*. In a Letter from* THOMAS YOUNG, *M. D. F. R. S. &c.*

To Mr. NICHOLSON.

SIR,

It is the opinion of Lagrange that the grave harmonics are not the reſults of imagination.

AT length Mr. Gough has accepted my invitation, and has adverted to the phenomena of the grave harmonics. Theſe ſounds, he thinks, are merely mental and imaginary; I ſuppoſe them real and material; Lagrange, whom I have already quoted, is of the ſame opinion; and while I have the authority of a man who is allowed to be either the firſt or ſecond phyſical mathematician in Europe, I ſhall be very unwilling either to diſbelieve my ears, or to confeſs that Mr. Gough has convicted me of error.

Developement of the theory by that author.

To ſhew that I have not advanced a theory ſo new as Mr. Gough has deemed it, I ſhall quote a paſſage from the firſt volume of the Miſcellanea Taurinenſia. "We have ſeen," ſays Lagrange, "that the particle of air which is found in a

* Philoſophical Journal, IV. page 1.

place

place where two ſounds meet, receives an agitation different from that which is produced by each ſound; if therefore the ſounds are of ſuch a nature that their vibrations coincide always after a certain given time, the continued and regular impreſſion of theſe compound agitations may be diſtinguiſhed from the ſimple agitations, and an ear ſufficiently exerciſed will hear a third ſound, of which the relation to the others may be found by comparing the number of ſeparate vibrations that each of them completes between two ſucceſſive coincidences," p. 103. "And the compound agitation may be conveyed to the ear in an infinite number of —— ſituations." p. 104. "We have already ſpoken of the beats of Mr. Sauveur, and we have ſeen that they correſpond exactly with the coincidences of the vibrations; there is therefore every reaſon to believe that they are formed in the ſame manner by the meeting of two ſounds. And it is probable that the third ſound of Tartini is only produced by a ſeries of theſe beats." P. 105.

If the grave harmonics be the reſult of compariſon; uniſons, of which the vibrations biſect each other, ought to give the octave;—but if they reſult from coaleſcence of undulations, the ſounds muſt here deſtroy each other.

Were the conteſt to be decided by authority, it is probable that your readers would prefer that of Lagrange to Mr. Gough's and mine united: but we have no occaſion for any thing more than reaſon and experiment. If the mind were capable of making up a ſound in the way that Mr. Gough ſuppoſed, we ought to hear, whenever the impulſes of one ſound biſect either accurately or very nearly, the intervals between the impulſes of another ſound, an imaginary note, an octave above the ſeparate ſounds: if, on the contrary, my opinion is true, we muſt conclude that the retrograde motions of the one will counteract the direct motions of the other, and that both the ſounds will be deſtroyed.

Experiment to ſhew that the latter fact obtains in nature.

Happily the point thus at iſſue may be determined by a very ſimple experiment: we have two ſounds ſtanding in this relation, in the intervals between the beats of two muſical chords tuned very nearly in uniſon. And if we liſten to a grave ſound which beats very ſlowly, while it is dying away, we ſhall obſerve, that in the interval of the laſt and fainteſt beats, when the ſound is leaſt mixed by reflections and irregular propagations, the note, inſtead of riſing to the octave, is wholly loſt.

The perception of a faint ſound from a tuning fork held be-

I confeſs with pleaſure, that Mr. Gough's explanation of the reſemblance which I have pointed out between the perception

tween the teeth will be attended with a beat if another found nearly unifon be tranfmitted through the air:

ception of a grave harmonic, and the fenfation of a ringing in the ears, is ingenious and probable; and I can mention a fingular experiment in confirmation of his opinion. If a tuning fork, faintly vibrating, be held between the teeth, and a found nearly approaching to the fame note, be tranfmitted through the air, the beating will be nearly as diftinct as if both founds arrived through the fame medium. From this circumftance, however, I only infer that all founds enter the ultimate organ of hearing nearly enough in the fame direction to produce an alternate intenfion and remiffion.

whence it is inferred the ultimate direction of all founds is nearly the fame.

The grave harmonics differ from primitive founds; but they no lefs real than compound tides, &c.

I allow that fuch a found differs from primitive founds in its want of appropriate direction, and in its mode of propagation. The daily tide at Batfha in Tunquin neither comes from the eaft nor from the weft, but it is as much a real tide as the grave harmonics are real founds. If any perfon fhould infift that the phenomenon is not a tide, but an alternate elevation and depreffion of the water only, and that it exifts only in the fenfations of the obfervers, and not in the fea, I fhould be very little difpofed to enter into arguments with him on the fubject.

Remarks on the proper vehicles of philofophical papers. Philof. Tranf.—Provincial Memoirs. Journals.

With refpect to the communication of my remarks to the Manchefter Society, I beg leave to reply, that if I thought a paper of permanent importance to the extenfion of fcience, I fhould confider myfelf as bound by my duty to the Royal Society and to pofterity, to offer it for infertion in the Philofophical Tranfactions; but if it were of a mere fuperficial and temporary nature, I fhould think it fufficient to publifh it in a refpectable literary Journal. I do not mean any difrefpect to provincial focieties, but many papers have appeared in the volumes of the Manchefter Memoirs, which would perhaps have been more fuitably placed in thofe of the Philofophical Tranfactions.

Your obedient,

humble fervant,

THOMAS YOUNG.

Jan. 10, 1803.

IV. *Experiments*

IV.

Experiments and Obſervations on the Power of Fluids to conduct Heat; with Reference to Count Rumford's Seventh Eſſay on the ſame Subject. By JOHN DALTON.

(*Concluded from Page* 58.)

COUNT Rumford's principal experiments are thoſe in which a cake of ice was confined on the bottom of a cylindrical glaſs jar, of 4.7 inches in diameter, and 14 high, and water poured upon it of different temperatures ſuffering it to ſtand, without agitation. He found that about 6lb of boiling hot water melted little more ice than as much water of 41°; and that by making ſuch allowances as the experiments ſeemed to warrant for deductions when hot water was uſed, water of 41°, or 9° above the freezing point, melted quite as much, and often more, than the hot water: From which he infers, that water. and by analogy all other fluids, do not tranſmit heat in the manner that ſolids do, but circulate it ſolely by the internal motion of their particles. Short recapitulation of Count Rumford's experiment of ice at the bottom of heated water.

The exiſtence of this internal motion he has proved decidedly; that water of a certain temperature being of the greateſt denſity, will always take the *loweſt* place, and water either warmer or colder than that degree will aſcend. This degree of greateſt condenſation he takes on the authority of others at 40°; it appears however from the experiment related above, to be ſtill more favourable to his poſition, namely $42\frac{1}{2}$°: and that water of 32° muſt aſcend till it comes to water of 53°, if it be not cooled in its progreſs, which circumſtance he admits. The internal motion or circulation.

Upon conſidering the facts related in his experiments therefore, there are *three* cauſes which ſuggeſt themſelves as conſpiring to circulate and diffuſe the heat, by which the ice is melted. Conſideration of the facts:

1ſt. The internal motion of the liquid, by which water of 32°, incumbent upon the ice, is perpetually aſcending into a warmer region of 53°, and warmer water of $42\frac{1}{2}$° deſcending to take its place. 1. Cold water at 32° riſes to the place of 53°, and warmer of $42\frac{1}{2}$° deſcends.

2d. The proper conducting power of the liquid independent of internal motion. 2. The fluid itſelf may conduct.

3d. The

3. The glafs itfelf may conduct; probably little.

3d. The conducting power of the glafs jar. But as glafs is known to be a very bad conductor of heat, it can produce no material effect in thefe experiments: for which reafon Count Rumford does not appreciate the third caufe.

Count Rumford does not allow that a cold fluid rifing into a warmer can cool it;

With refpect to the operation of the firft caufe, it will *generally* be fuppofed that *cold* water rifing into *warmer* and remaining with it, the heat is impaired, and the two reduced to a common temperature. But Count Rumford does not admit of this communication; he maintains, that the two ftill retain their proper fhare of heat, notwithftanding they are mixed together. This hypothefis of his is of no peculiar confequence as far as refpects the effect of the internal motion: for the temperature indicated by a thermometer immerfed in an equal mixture of water at 32° and 53°, would be the fame as if the water was uniformly of the temperature $42\frac{1}{2}$°. But it has

whence it would follow that heat cannot by tranfmiffion downwards throw water above $42\frac{1}{2}$°.

material confequences in other refpects; for, if it be admitted, it annihilates the *fecond* caufe abovementioned, and it would follow that warm water being put upon cold water above the temperature of $42\frac{1}{2}$°, the heat could not in any degree be propagated downwards, unlefs by agitation, and even then, upon fubfiding, the warm part ought to rife to the top, and the cold fall to the bottom.

He has not proved this by experiment.

Thefe pofitions are fo manifeftly contradictory to common opinion, that they cannot be received without proof. But Count Rumford has not given us a fingle experiment to prove them. It feemed neceffary therefore, to clear up this point by direct experiments.

Experiment 1.

Examination by Exper. 1. Hot water poured upon cold air in a glafs veffel, the upper part gradually cooled, and the lower heated.

Took a large tumbler glafs, $3\frac{1}{2}$ inches diameter, and five inches deep, and filled it half way with water of 51°, then gently filled up the reft by means of a fmall fyphon, with water of 88°; a thermometer, with its bulb and ftem detached from the frame, being previoufly immerfed to the bottom. The temperatures at the top and middle were had by gently immerfing the bulb of another thermometer into the water.

Time

Time elapsed.	TEMPERATURE *at top.*	*in the middle.*	*at bottom.*
—	88°	—	51°
5 min.	85	—	54
12	83	75°	56
18	80	72 +	58
30	76	—	60
40	73	—	61
50	70	—	61
60	69	—	61

(Air in the room 50°.)

Experiment 2.

The same as before; only a circular piece of wood floated upon the surface of the water, on the centre of which the stream of the syphon was directed to prevent the current downwards.

Exp. 2. The same with more precaution.

(Air in the room 55°.)

Time.	TEMPERATURE. *at top.*	*at bottom.*
Before the water was poured on	....	56°
—	116°	56 +
10 min.	105	56½
20	92	57 —
30	85	57⅓
40	80	57¾
50	77	58 +
1 h. —	75	58½
— 10	72½	58½
— 20	70	59 —
— 30	67	58½
— 40	65	58 +
— 50	63	58
2 —	62	58 —
— 15	61	57½
— 30	59½	57
3 —	57½	56 (Air 52°)
5 —	53½	53 Do.

A similar result was obtained in a different way by the following.

Experiment

Experiment 3.

3. The part of [w]ater heated by hot iron; [th]e lower part [be]came alſo hot-[te]r.

Took an ale glaſs of a conical figure, $2\frac{1}{2}$ inches in diameter and 3 inches deep; filled it with water that had been ſtanding in the room, and conſequently of the temperature of the air nearly—Put the bulb of a thermometer to the bottom of the glaſs, the ſcale being out of the water: Then, having marked the temperature, I put the red hot tip of a poker, half an inch deep into the water, holding it there ſteadily about half a minute; and as ſoon as it was withdrawn, I dipt the bulb of a ſenſible thermometer into the water about $\frac{1}{4}$ inch, when it roſe in a few ſeconds to 180°.

	TEMPERATURE		
Time.	*at top.*	*middle.*	*bottom.*
Before the poker was immerſed			47°
—	180°	—	47
5 min.	100	60°	$47\frac{1}{2}$
20	70	60	49
1 h. —	55	—	52

Theſe experiments all evidently agree in proving water to have a proper conducting power, independent of any internal motion. It ſurely will not be ſaid that any ſlight motion unavoidably made at the beginning of an experiment, could continue with a powerful effect for upwards of an hour. However, to determine this matter, I made the two following experiments.

Experiment 4.

Exp. 4. Coloured water was heated by hot colourleſs water reſting upon it, without mixture.

Took the glaſs tumbler of the firſt experiment, and filled it half way with rain water, deeply tinged with archil; then filled it up with clear warm water, as related in the 2d experiment. The upper half was but juſt perceptibly tinged by the proceſs and uniformly ſo; it remained for an hour not viſibly altered in this reſpect, though by frequently putting the bulb of a thermometer down to the middle, the colour at laſt roſe in a ſmall degree.

(Air

(Air 45°)

TEMPERATURE

	at top.	middle.	bottom.
Before the warm water was poured on			44°
Time	105°	—	—
7 min.	97	—	47+
17	86	—	48
27	79	—	49+
37	75	68	50
47	70	66	50+
57	66	62	51½
1 h. 7	60	62	51½
— 17	60+	59	51½
— 27	59	—	51½

Experiment 5.

Warm coloured falt and water was cooled by cold frefh water refting on it without mixture.

A glafs tube near an inch in diameter, and 16 inches long, was half filled with a coloured folution of common falt in water, warm; a fmall thermometer was wholly immerfed in it, and cold clear water carefully poured upon the whole fo as to fill the tube; the colour afcended very little, and continued invariable after the procefs of filling. The warm folution was of courfe made of greater fpecific gravity than the cold water.

(Air 45°)

TEMPERATURE

Time.	at top.	bottom.
	45°	85°
5 min.	53	79
10	53	74
21	52	69
31	51	66
45	50½	64
58	50	61
1 h. 31	49	56½
3 30	47	51
— 55	47	50
4 15	—	49
7 5	46	48

To

To determine whether hot and cold water being fuddenly mixed, and agitated, the hot would afterwards rife to the top, was the object of

Experiment 6.

. 6. Hot & cold water being mixed, did not afterwards feparate.

Air in the room 50°.—About ½ pint of water of 130° was poured into a cold tumbler glafs, and immediately after as much water of 50°; the mixture was agitated for half a minute by a deal rod; after which an immerfed thermometer ftood at 85°, both at top and bottom; it was then fet by in a ftill place for examination.

	TEMPERATURE	
Time.	*at top.*	*bottom.*
15 min.	77¼°	77°
30	73	72⅔
45	68	67⅔
1 h. —	64.8	64.6

From all which water is concluded to be a proper conductor.

From all thefe experiments it is evident, that water has a proper conducting power: In the laft experiment, if the particles of water during the agitation had not actually communicated their heat, the hot ones ought to have rifen to the top, and the cold ones fubfided fo as to have made a material difference in the temperature. It is, however, equally evident, that water is a *bad conductor* of heat, probably as it is of electricity; the defcent of the heat in the fecond experiment is wonderfully flow; a flight agitation for one *fecond* would do as much to induce the equilibrium as ftanding ftill one *hour*. In repeating the third experiment, in a wine glafs, I have feveral times known water ½ an inch deeper to differ 50° in temperature from the incumbent water.

We muft conclude, therefore, that the quick circulation of heat in water over a fire, &c. is owing *principally* to the internal motion excited by an alteration of fpecific gravity; but not *folely* to that caufe as Count Rumford has inferred.

If it be proved that water conducts heat, it will fcarcely be neceffary to prove, that other fluids conduct it, and that they communicate it one to another:—The two following experiments fhew that mercury conducts it, and that water and mercury reciprocally communicate it.

Experiment

Experiment 7.

Took a cylindrical glafs tube, of 1 inch internal diameter, and put 1¼ inches in depth of mercury into it, and immerfed the bulb and ftem of a thermometer to the bottom, the fcale as ufual being above the liquid; then put 2¼ inches of warm water upon it by a fyphon, and let it ftand without agitation.

Exp. 7. Hot water, refting on mercury, heated it.

TEMPERATURE. TEMPERATURE.

Time.	Merc.	Water.	Time.	Merc.	Water.
	56°	122	14 m.	74½°	92°
3 m.	70	118	19	73	87
6	73	110	27	71	78
11	75	100			

Experiment 8.

Into a tumbler glafs, 2½ inches in diameter, poured an inch in depth of mercury, and heated it to 110°; upon which was poured an inch of water at 50°, and then kept ftill.

Exp. 8. Cold water upon hot mercury, was heated.

TEMPERATURE.

Time.	Merc.	Water.
	110°	50°
4 min.	74	70
8	71	70½
10	70½	70

Finding that water was fo bad a conductor of heat, I was defirous to learn how ice would conduct it, and tried it as follows.

Experiment 9.

Feb. 9th. Out of a mafs of ice, by means of a hot iron, I fhaped a cylindrical piece, 3 inches in diameter, and 5½ inches long, clean and pure; its weight 17 ounces. Made a fmall round hole at one end, one inch deep, and the fize of a thermometer bulb, which was inclofed in it. The other end of the piece was put into a bafon of fnow and falt, to the depth of from ½ to 1½ inches, the temperature of which was kept below 10° for 1¾ hours. Air 37°.

Ice, immerfed partially in the freezing mixture, conducted very badly,—much worfe than water.

Time elapfed.	Therm. in the liquid.	Therm. in the ice.
	5°	32°
1¾ h. at a medium	7	31½

N. B. This defcent of half a degree was gradual, but did not commence till long after the beginning of the experiment. After this the piece of ice was inclined to one fide, by which nearly one half of it was immerfed in the cooling liquid, and the inclofed bulb of the thermometer was now not more than an inch from the cold mixture.

H.	M.	Therm. in the liquid.	Therm. in the ice.
1	50*	14°	28°
2	20	19	28
2	50	22	Ice along with the therm. flipped down into the cold liquid.

The ice now weighed $12\frac{1}{2}$ ounces: the reft had been liquified by the operation of the faline liquor.

This experiment, I think, decidedly proves that ice is a worfe conductor of heat than water: indeed this is not wonderful; for it is faid, that ice at a low temperature becomes an electric.

Mixture is much more effectual for equalizing temperature, becaufe hot and cold particles are brought together.

It is certainly a remarkable circumftance, but not at all inconfiftent with the known laws of heat, that in a mixture of hot and cold liquids, the uniform temperature fhould be fo foon induced by agitation and fo flowly by reft: but when we confider, that in the former cafe, hot and cold particles are brought together, and that in the latter there is a feries of particles one upon another, gradually rifing in temperature, but differing by infenfible degrees, we fhall not wonder at the facts. When any one particle of water, or any other body, has one above it, warmer by an infenfible degree, and another below it, colder by an infenfible degree, its power to tranfmit heat muft be very fmall †. Thefe confiderations gave rife to the two following experiments.

The gradual difference of temperature renders water a bad conductor.

Experiment 10.

Exp. 10. An heated mercurial thermometer expofed to air, gave out moft heat when at the higheft temperatures.

A mercurial thermometer was taken, its bulb $\frac{4}{10}$ inch in diameter, and hanging clear of the fcale: it was heated by the flame of a candle to 600°, and then laid upon a table with the

* From the beginning of the experiment.

† Will not this argument apply to folids univerfally? contrary to fact. N.

the bulb projecting over the edge, and was thus left to cool by the mere operation of the air in the room, which was 52°. The following is the medium refult of two experiments, which, however, agreed with each other almoft in every obfervation.

Time.	*Temp.*	*Time.*	*Temp.*
0	600°	18 half m.	66°
1 half m.	450	19	64
2	350	20	62
3	280	21	60
4	229	22	59
5	195	23	58
6	168	24	57
7	145	25	56
8	128	26	55
9	115	27	54
10	104	28	53+
11	95	29	53
12	88	30	53
13	81	31	53
14	77	32	52+
15	73	33	52
16	69+	Air in the room	52
17	68		

Experiment 11.

Another thermometer, having a fimilar bulb, but a fcale with much larger degrees, was heated and cooled in the fame manner.

Time.	*Temp.*	*Time.*	*Temp.*
—	85°	16	56.3°
1 half m.	$79\frac{1}{2}$	17	$56\frac{1}{4}$
2	75	18	56
3	71+	19	55.9
4	$68\frac{1}{2}$	20	55.7+
5	$66\frac{1}{3}$	21	55.6+
6	$64\frac{1}{2}$	22	55.5
7	63	23	55.4
8	$61\frac{1}{2}$	24	55.3
9	60	25	55.25
10	59	26	55.2—
11	$58\frac{3}{4}$	27	55.1+
12	58	28	55.1—
13	$57\frac{1}{4}$	29	55.+
14	57	30	55
15	56.6		

In theſe experiments we may conſider mercury and air mixed together of unequal temperatures, with a thin partition of glaſs—and from the laſt we may conclude, that the thermometer imparted to the air 40 times more heat in half a minute, when its temperature was 30° above the air, than when it was only 1° above it.

Argument from the Count's own experiments. No water but that below 53° could deſcend to the ice, and conſequently this only would be cooled, and the fuſion of the ice would then ceaſe.—Contrary to fact.

We ſhall now advert a little to Count Rumford's experiments. It will eaſily appear, that arguing fairly upon his own hypotheſis he can never account for the phenomena obſerved: for, hot water being poured upon ice, an internal motion would take place near the ſurface of the ice, by which a ſtratum of water of a certain thickneſs would be reduced to 32°, and then all further reduction of the ice muſt ceaſe; becauſe all the ſuperincumbent water being above 53° would be lighter and could not deſcend to the ice. But this is quite contrary to what took place. The facts, however, will admit of a ſatisfactory explanation upon eſtabliſhed principles.

Explanation from the known facts. The ice is fuſed (1) by the proper conducting power of the water; and (2) by its motion. The firſt is as the heat, and the ſecond diminiſhes in ſome unknown ratio as the temper. differs from $42\frac{1}{2}$.°

By experiments 10 and 11, it appears, that the quantity of heat given out by a body, during any ſmall given portion of time, is nearly as the exceſs of the temperature of the body above the cooling medium. Hence, then, we may conclude, that the effect of hot water upon ice ariſing from the proper conducting power of water, will be nearly as the heat of the water. What effect the other cauſe may produce, it will be difficult to determine from theory: experience will be the beſt guide. One thing, however, appears pretty certain, that its effect muſt be a *maximum*, when the temperature of the water at large is $42\frac{1}{2}$° ; becauſe then there can never want a determination of the particles *downward* to ſupply the place of the lighter water of 32° aſcending. If the temperature of the water exceed $42\frac{1}{2}$°, then the effect of the internal motion will be leſs, diminiſhing by ſome unknown ratio. As far as I can judge from Count R's experiments, the joint effects of thoſe two cauſes ſhould be nearly the ſame with water of 42° and water of 190°. Taking this, therefore, for granted, we ſhall be enabled to ſketch a table of the values of theſe two cauſes for every 10° of temperature. The numbers expreſſing the effect of the proper conducting power, are derived from the 10th experiment, and conſequently are not purely hypothetical: thoſe expreſſing the other effect, except 42° and 192°, are put down hypothetically, becauſe the law of decreaſe has not been aſcertained.

Water at $42\frac{1}{2}$° appears to fuſe (chiefly by its motion) nearly the ſame as water at 190° (chiefly by its heat.)

4 It

It is to be ſuppoſed, that a given quantity of water, of the ſeveral temperatures mentioned, is carefully poured upon a cake of ice at the bottom of a cylindrical glaſs jar, and ſtands without agitation for a given time, as half an hour; then the proportionate quantity of ice ſuppoſed to be melted by the two cauſes ſeparately are ſtated in numbers, and then the ſums are taken to expreſs the joint effects.

The reſults ſketched out for different temperatures.

Temperature

Temperature of the water on the ice.	32.°	42.°	52.°	62.°	72.°	82.°	92.°	102.°	112.°	122.°	132.°	142.°	152.°	162.°	172.°	182.°	192.°	202.°	212.°
Ice melted by the conducting power of the water.	0.	2.	3.	6.	9.	11.	12.	14.	17.	19.	21.	23.	25.	27.	30.	33.	35.	38.	40.
Ice melted by the internal motion of the particles of water.	0.	36.	35.	31.	25.	19.	16.	14.	12.	10.	8.	7.	6.	5.	4.	3.	3.	3.	3.
Total quantity melted by both causes.	0.	38.	38.	37.	34.	30.	28.	28.	29.	29.	29.	30.	31.	32.	34.	36.	38.	41.	43.

After what has been ſaid, I need not caution my readers not to conſider this table as accurate. The principle of it, however, cannot I conceive be diſproved: that the operation of the conducting power muſt be proportionate to a ſeries of numbers beginning from 0 at 32°, and gradually increaſing in ſome ratio with the temperature above 32°, cannot, I think, be controverted; and that the operation of the internal motion muſt begin from 0 at 32°, and increaſe till it arrives at its *maximum* at 42½°, and then decreaſe again ever after, is alſo, I apprehend, unqueſtionable: thus, when the jar had water of 42°, in Count R's experiments, this internal motion muſt have had a range of 8 inches in depth; whereas, when hot water alone was uſed, it had not more than ¾ of an inch to range from the temperature of 32 to that of 53°.

Obſervations on the table.

The following table exhibits a conciſe view of all the material varieties of Count Rumford's experiments, with their reſults.

Table of the varieties of the Count's experiments.

Experiment

	Temperature of the water when poured on the ice. At the beginning	end	Medium Temperature.	Water in the jar surrounded by		Ice melted in 30 minutes.
Experiment 32	41°	40°	$40\frac{1}{2}$°	Air	41°	617 Grains
35	189	180	$184\frac{1}{2}$	*a warm covering of cotton wool.*		747
38	41	43	42	Air	61	676
39	180	157	$171\frac{1}{2}$	Air	61	559
45	188	64	128	Ice & water	32	406
51	61	49	55	Ice & water	32	660
53	61	60	$60\frac{1}{2}$	Air	61	642

Count Rumford attempts to explain why there was lefs ice melted in fuch experiments as the 45th than in thofe like the 39th, and attributes the diminution of the effect to the defcending currents, occafioned by the cold mixture furrounding the warm one, which he thinks would obftruct the oppofite ones afcending from the ice. But the effect in the 51ft, compared with the 53d, being juft oppofite, he paffes over without explanation.—I have no doubt myfelf, but that the true caufe of the differences in both cafes, is to be found in the column expreffing the *mean temperature* of the water, and not in that expreffing its fituation, which I confider as having nothing to do in the bufinefs, but as it affects the general temperature. The *maximum* effect with cold water will be when it is of the temperature of about 48° or 50°, and the *minimum* above it probably about 100° or 120°; and in proportion as the mean temperatures, in any experiment, deviate from thofe points, the effects vary accordingly, let other circumftances be what they may.

Remarks.

The differences depend on the mean temperature of the water.

Thus I have attempted to explain the rationale of thefe very curious and interefting experiments, in a manner different to what their ingenious author has done. And muft now leave it to the reader to form his opinion.

Conclufion.

V.

A Method of examining refractive and difperfive Powers, by prifmatic Reflection. By WILLIAM HYDE WOLLASTON, *M. D. F. R. S.* *

IN examining the power with which various fubftances refract and difperfe light, I have for fome time paft employed a method unnoticed by writers on optical fubjects; and, as it is not only convenient in common cafes of refraction, but alfo capable of affording refults not attainable by other means, I have been induced to draw up a fhort account of the method itfelf, and of the moft remarkable inftances of its application.

New method of examining the refracting and difperfing powers of bodies, upon light;

This method was fuggefted by a confideration of Sir Ifaac Newton's prifmatic eye-glafs, the principle of which depends on the reflection of light at the inner furface of a denfe refracting medium.

Since the range of inclination within which total reflection takes

by angle of total reflection at the

* Philof. Tranf. 1802.

confine of the denſer of two mediums: takes place, depends not only on the denſity of the reflecting priſm, but alſo on the rarity of the medium adjacent to it, the extent of that range varies with the difference of the denſities of the two media. When, therefore, the refractive power of one medium is known, that of any rarer medium may be learned, by examining at what angle a ray of light will be reflected from it.

General fact ſtated. The angle is greater, the greater the refractive denſity of the medium towards which the light paſſes; For inſtance, when any object is laid under a priſm of flint-glaſs, with air alone interpoſed, the internal angle of incidence at which the viſual ray begins to be totally reflected, and at which the object ceaſes to be ſeen by refraction, is about 39° 10′; but, when the object has been dipped in water, and brought into contact with the glaſs, it continues viſible, by means of the higher refractive power of the water, as far as $57\frac{1}{2}°$ of incidence. When any kind of oil, or any reſinous cement, is interpoſed, this angle is ſtill greater, according to the refractive power of the medium employed; and, by cements that refract more ſtrongly than the glaſs, the object may be ſeen through the priſm, at whatever angle of incidence it is viewed. until total tranſmiſſion.

Fluids are eaſily applied to the denſe medium (or glaſs): But ſolids require the interpoſition of a fluid more refringent than themſelves. In examining the refractive powers of fluids, or of fuſible ſubſtances, the requiſite contact is eaſily obtained; but, with ſolids, which can in few inſtances be made to touch to any great extent, this cannot be effected without the interpoſition of ſome fluid, or cement, of higher refractive power than the medium under examination. Since the ſurfaces of a ſtratum ſo interpoſed are parallel, it will not affect the total deviation of a ray paſſing through it, and may therefore be employed without riſk of any error in conſequence.

Thus, reſin, or oil of ſaſſafras, interpoſed between plate glaſs and any other priſm, will not alter the reſult.

A triangular glaſs priſm may be uſed for comparing the powers of two bodies; If, on the ſame priſm, a piece of ſelenite and another of plate-glaſs be cemented near each other, their powers may be compared with the ſame accuracy as if they were both in abſolute contact with it.

For ſuch a mere compariſon of any two bodies, a common triangular priſm is beſt adapted; but, for the purpoſe of actual meaſurement of refractive powers, I have preferred the uſe of a ſquare priſm, becauſe, with a very ſimple apparatus, it ſhows the ſine of refractive power ſought, without the need of any calculation. but a ſquare priſm ſhews the ſine of refraction without calculation.

Figure and application of the ſquare priſm. Let A, Fig. 1, Plate IV. be a ſquare or rectangular priſm, to which

which any ſubſtance is applied at *b*, and let any ray of light parallel to *c b* be refracted through the priſm, in the direction *b d e*.

Then, if *e f* and *e d* be taken proportional to the ſines that repreſent the refractive powers of the priſm and of air, *f g*, which is intercepted between *f* and the perpendicular *e g*, will be the correſponding ſine to repreſent the refractive power of the medium *b*. For, ſince *e d g* (oppoſite to *e f*) is the angle of refraction, *e f g* (oppoſite to *e d*) muſt be equal to the angle of incidence *b d h*; and *e f* : *f g* : : *b d* : *d h* : : ſine of *c b i* : ſine of *h b d*.

Conſtruction to ſhew the ſine of refraction from any given rarer body into glaſs.

All therefore that is requiſite for determining the refractive power of *h*, is to find means of meaſuring the line *f g*. On this principle, the inſtrument in the annexed ſketch (Fig. 2.) is conſtructed. On a board *a b* is fixed a piece of flat deal *c d*, to which, by a hinge at *d*, is jointed a ſecond piece *d e*, 10 inches long, carrying two plane ſights at its extremities. At *e* is a ſecond hinge, connecting *e f*, 15,83 inches long; and a third at the other extremity of *e f*, by which *f g* is connected with it. At *i* alſo is a hinge, uniting the radius *i g* to the middle of *e f*; and then, ſince *g* moves in a ſemicircle *e g f*, a line joining *e* and *g* would be perpendicular to *f g*.

Inſtrument giving the ſine by inſpection.

The piece *c d* has a cavity in the middle of it, ſo that, when any ſubſtance is applied to the middle of the priſm P, it may continue to reſt horizontally on its extremities. When *e d* has been ſo elevated that the yellow rays in the fringe of colours (obſervable where perfect reflection terminates) are ſeen through the ſights, the point *g*, by means of a vernier which it carries, ſhows by inſpection the length of the ſine of refraction ſought.

The advantages which this method poſſeſſes above the uſual mode of examining refractive powers, are greater than they may at firſt ſight appear. The uſual practice has been, to form two ſurfaces of the ſubſtance under examination, ſo inclined to each other that the deviation occaſioned by them might be meaſured. The inclination of theſe ſurfaces to each other muſt alſo be known; and thence the refractive power might be computed. But, in the method here propoſed, it is ſufficient to have only one ſurface, and the reſult is obtained at once, without computation.

This method is moſt accurate and eaſy: it requires only one ſurface and gives the reſult at ſight.

The facility of determining refractive powers, is conſequently ſuch as to render this property of bodies a very convenient teſt in many philoſophical inquiries. For diſcovering the purity of eſſential oils, ſuch an examination may be of con-

Its facility renders it uſeful as a teſt in philoſophical inquiries.

 ſiderable

ſiderable utility, on account of the ſmallneſs of the quantity requiſite for trial. In oil of cloves, for inſtance, I have met with a wide difference. The refractive power of genuine oil of cloves, is as high as 1,535; but I have alſo purchaſed oil by this name, which did not exceed 1,498, and which had probably been adulterated by ſome leſs refractive oil.

It is applicable to opake bodies.

For ſuch purpoſes, the refractive power of opaque ſubſtances may often be deſerving of inquiry, which could not be learned by any means at preſent in uſe. For, in the uſual mode, a certain degree of tranſparency is abſolutely neceſſary; but, for trial by contact, the moſt perfect opacity does not occaſion the leaſt impediment.

Inſtance.

Among other inſtances in which I have taken advantage of this circumſtance, I may mention a ſubſtance that had been found in one of the iſlands of the North Pacific Ocean, which, to all outward appearance and by various trials, ſeemed to be perfect bees-wax, although it is ſuppoſed that there are no bees in the iſland from which it was brought. On placing it by the ſide of a piece of bees-wax, in contact with a priſm, the perfect equality of their refractive powers afforded a ſtrong confirmation of the opinion before formed of their identity.

Bodies of varying denſity may be examined by it,

For the examination alſo of media of which the refractive denſity is not uniform, the general method of trial by deviation wholly fails; on the contrary, by placing a varied medium in contact with a priſm, all its gradations of denſity, from greateſt to leaſt, become at once the object of mere inſpection. An inſtance of this may very readily be ſeen with a piece of gum, the ſurface of which has been moiſtened for a few minutes; when, by cloſe application to a priſm, a refractive power may be diſcerned, varying from that of the water on the ſurface, 1,336, to nearly 1,51, the refractive power of gum arabic.

particularly the cryſtalline lens.

I ſhould not ſo much inſiſt on this advantage, were it not for the opportunity hereby afforded of examining the cryſtalline lens of the eye, which is now known to be generally more denſe in the centre than at its ſurface.

Mr. Haukſbee, who was not aware of this difference, has eſtimated the refractive power of the cryſtalline lens, by forming it into a wedge by plates of glaſs, ſomewhat higher than I find it to be; but, with his accuſtomed accuracy, he remarked the apparent enlargement of an object, occaſioned by the variations of its denſity, which he was unable to explain.

In the table that follows, I have ſet down, not only the limits of

of refractive power in a cryſtalline lens of an ox, aſcertained by trial, but alſo an average, computed from the refractive denſity of a dried cryſtalline of an ox, of which the weight had been firſt taken in the recent ſtate, and the quantity of water loſt by drying alſo meaſured.

The table exhibits a ſeries of ſubſtances, arranged according to their refractive powers. That of the diamond is copied from Sir Iſaac Newton; of other bodies to which (on account of their being more denſe than glaſs) the machine for meaſurement would not apply, the refractive powers have been found by other means, for the ſake of furniſhing a more continued ſeries of ſubjects for comparative experiments. The reſt have been compared by this method; and their power, when expreſſed in numbers, actually meaſured. Table of refractive powers.

TABLE I.

Diamond - -	2,44	Oil of ſaſſafras -	1,536
Plumbago - -	—	Red ſealing-wax	—
Native ſulphur (double)	2,04	Spermaceti, cold	—
Glaſs, conſiſting of lead 6 and ſand 1 -	1,987	Sugar, after fuſion	—
Glaſs of antimony -	1,98	Arſeniate of potaſh	—
Jargon - -	1,95	Maſtic	—
Spinelle ruby -	1,812	Elemi	—
Arſenic - -	1,811	White wax (cold)	—
Muriate of antimony, variable		Oil of cloves -	1,535
White ſapphire -	1,768	Copal - - -	1,535
Gum dragon - -	—	Anime - -	1,535
Iceland ſpar, ſtrongeſt	1,657	*Radcliffe crown glaſs*	1,533
Sulphate of barytes (double) - -	1,646	Pitch	—
Balſam of Tolu -	1,60	Centre of cryſtalline of fiſh, and dry cryſtalline of an ox -	1,530
Guaiacum - -	1,596	Canada balſam -	1,528
Benzoin - -	—	*Crown glaſs, common*	1,525
Flint glaſs - -	1,586	Selenite - -	1,525
Ditto - - -	1,583	Caoutchouc -	1,524
Horn	—	Gum lac	—
Phoſphorus - -	1,579	*Dutch plate glaſs* -	1,517
Mica	—	Human cuticle -	—
Opium	—	Gum arabic -	1,514
Amber - -	1,547	Balſam of capivi -	1,507
Rock cryſtal (double)	1,547	Oil of amber -	1,505
Old plate glaſs -	1,545	*Engliſh plate glaſs* -	1,504
Colophony - -	1,543	*French plate glaſs* -	1,500
Box-wood	—	Oil of nutmeg -	1,497
Bees-wax - -	1,542	Sulphate of potaſh	1,495

Tallow

Tallow, cold -	1,49	Cryſtalline lens of an ox	1,447
Iceland ſpar, weakeſt	1,488	to -	1,380
Camphor - -	1,487	Computed average of ditto - -	1,430
Linſeed oil - -	1,485	Sulphuric acid -	1,435
Butter, cold -	1,480	Fluor ſpar - -	1,433
Eſſence of lemon -	1,476	Nitric acid (ſp. gr. 1,48)	1,410
Oil of turpentine, common - -	1,476	Alcohol - -	1,37
——— rectified	1,470	White of an egg -	1,36
Oil of almonds	—	Æther - -	1,358
—— olives -	1,469	Vitreous humour of an eye - -	1,336
—— peppermint -	1,468	Water - -	1,336
—— lavender -	1,467	Atmoſpheric air (Haukſbee) -	1,00032
Tallow, *melted* -	1,460		
Alum - - -	1,457		
Spermaceti, *melted* -	1,446		

ON THE DISPERSION OF LIGHT.

This method is likewiſe applicable to ſhew the diſperſion of light.

The method above deſcribed for inveſtigating refractive powers, may alſo be employed with ſimilar advantage for inquiries into the diſperſion of light by different bodies, and the conſequences that reſult from their combined action.

The limit of perfect reflection is marked by a fringe of colours;

When a glaſs priſm is placed in contact with water, and brought near the eye, in ſuch a poſition that it reflects the light from a window, the extent of perfect reflection is ſeen to be bounded by a fringe of the priſmatic colours, in the order of their refrangibility.* The violent rays, being in this caſe the moſt refrangible, appear ſtrongeſt and loweſt, on account of the leſs obliquity that is requiſite for their reflection.

except when (the refractions being unequal) the diſperſions are equal.

But it may happen that two media, which refract unequally at the ſame incidence, may diſperſe equally at that incidence. Under theſe circumſtances, a pencil of rays paſſing from one of ſuch media into the other, will be refracted, without diſperſion of its colours. The boundary of priſmatic reflection would then be found a well defined line, free from colour, if the ſurface at which the reflected light emerges from the priſm, were at right angles to its courſe.

If the diſperſion by the rarer medium be greateſt the order of colours will be inverted.

When the diſparity of the diſperſive powers of the media is ſtill greater, it may alſo happen, that the uſual order of priſmatic colours will be reverſed; and then the red will appear ſtrongeſt and loweſt in the fringe, unleſs the colours ſo pro-

* Newton's Optics. Book i. part 2. Exp. 16.

duced

duced are counteracted by refraction at their emergence from the prifm.

An inftance in which the colours are fo reverfed, may be feen by application of oil of faffafras to a prifm of flint glafs.

Inftance: F. glafs and ol. faffafras.

So high is the difperfive power of this oil, that, in refractions from flint glafs into it, the red rays are refracted more than the violet.

It muft be obferved that, in this experiment, when the angle of reflection within a triangular prifm exceeds 60°, the angle of emergence is fuch as would alone occafion the red rays to appear lowermoft; but, when the glafs ufed is rectangular, the refraction at emergence has an oppofite effect; any reverfion of colour will therefore be in fome degree corrected, and may not be feen, unlefs the difperfive power of the medium in contact much exceeds that of the glafs.

A cafe of refraction with an inverted order of colours, has been obferved by Dr. Blair,* in a compound object-glafs where crown-glafs was in contact with oil of turpentine. From trials with lenfes, he likewife inferred, that feveral other fluids have the fame effect, when applied to that glafs.

Crown glafs and oil of turpentine.

With this glafs, and alfo with plate-glafs, I have tried oil of turpentine, and many other fluids that afford a fimilar reverfion of colours, as linfeed-oil, olive-oil, the effential oils of bergamot, lemon, lavender, pennyroyal, and peppermint, ftrong nitric acid, and many artificial compounds that I fhall prefently have occafion to mention.

other oils, &c.

The difperfive power of fluor fpar is the leaft of any fubftance yet examined; fo that, although its refractive power is alfo remarkably low, (confidering its great fpecific gravity,) a prifm of fluor, in contact with water or alcohol, fhows the prifmatic colours to be refracted in an inverted order.

Fluor fpar difperfes very little,

With heavy fpar, the inftances of reverfion are very numerous, as its difperfive power is low, and is accompanied with great refractive denfity. In the refractions from this fpar into flint glafs, and into all oils or refins, I believe, without exception, the colours are feen reverfed.

alfo ponderous fpar

Rock cryftal likewife difperfes fo little, that it exhibits the colours reverfed, when it is in contact with many fubftances of lefs refractive power than itfelf. I have tried it with Dutch plate-glafs, with Canada balfam and balfam of capivi, with

and rock cryftal

* Edinb. Tranf. Vol. III.

many

many oils eſſential and expreſſed, and have found the colours in all theſe caſes reverſed.

Metallic ſalts diſperſe much.

By ſolutions of metallic ſalts, a great variety of ſuch appearances may be produced. Moſt of theſe compounds have a highly diſperſive power; and many of them may be rendered ſufficiently denſe to occaſion reverſion, even when applied to flint-glaſs. In a more dilute ſtate, they may be uſed with crown-glaſs, or plate-glaſs, to produce the ſame effect. And ſince, when further diluted by a leſs diſperſive medium, they will alſo preſent an appearance of colourleſs refraction, we may, by examining the degree of dilution neceſſary for that purpoſe, compare the diſperſive powers of any ingredients contained in them, and may gradually extend our knowledge of this property to the elements of any bodies, however compounded.

Experiments of diluting them till no colour appeared.

As a ſpecimen of the method, I have in this way compared a few ſolutions of metals, and of other ſubſtances, that were each diluted till the limit of reflection appeared void of colour, when they were in contact with a rectangular piece of plate-glaſs; and, in the table which follows, I have expreſſed their refractive powers in that ſtate of dilution, as nearly as the eye can diſcern the diſappearance of colour.

TABLE II.

Table of their denſities in that ſtate.

	In Water.	In Alcohol.
Nitro-muriate of gold - - - -	1,364	1,390
Nitro-muriate of platina - -	1,370	
Nitrate of iron - - - - -	1,375	
Sulphuret of potaſh - - -	1,375	
Red muriate of iron - - -	1,385	
Nitrate of magneſia		
Nitric acid - - - - - -	1,395	
Nitrate of jargon		
Balſam of Tolu - - - - -	—	1,400
Acite of litharge (extract of lead) -	1,400	
Nitrate of ſilver		
Nitrate of copper		
Oil of ſaſſafras - - - - -	—	1,405
Muriate of antimony - - -	—	1,410
Nitrate of lime - - - - -	1,410	1,422
Nitrate of zinc		
Green muriate of iron - - -	1,415	
Muriate of magneſia - - - -	1,416	
——— of lime - - - - -	1,425	1,440
——— of zinc - - - - -	1,425	
Eſſence of lemon - - - - -	—	1,430
Balſam of capivi - - - - -	—	1,440

It

It may here be feen, that feveral of the metals increafe the difperfive powers of nitric and muriatic acids, and confequently exceed them in that refpect. Of all thefe fubftances that I have yet tried, gold and platina are the moft difperfive. The leaft difperfive of the metals is zinc.

Gold and platina moft increafe the difperfion; zinc leaft.

The earths alfo are found to poffefs this property in very different degrees: that of the jargon and magnefia differ but little from nitric acid in difperfive power; but filiceous earth, on the contrary, is inferior to water.

The earths differ much.

By comparing the falts formed with the nitric and muriatic acids, it appeared probable that the former had the higher difperfive power; but a more direct comparifon could not be made by means of the rectangular piece of plate-glafs, as muriatic acid could not be rendered fufficiently denfe for fuch a trial; I therefore made ufe of a triangular prifm of crown-glafs, which is in itfelf lefs difperfive than any plate-glafs, and, from the relative pofition of its furfaces, occafioned lefs correction of the colours. With this prifm, I found that ftrong muriatic acid (having a refractive power 1,394) exhibited the colours reverfed; and that, when it was diluted till the limit of reflection appeared void of colour, its refractive power was reduced to 1,382. But the difperfive power of nitric acid, when tried by the fame prifm, proved to be greater; for this acid required to be diluted till its refractive power did not exceed 1,375, before the colour was wholly deftroyed.

Nitric acid difperfes more than muriatic.

In the table it may be obferved, that the red and green muriates of iron, though confifting of the fame metal and acid, differ very much in difperfive power; and, confequently, that fome caution will be neceffary, in attempting to compare the different metals with each other by means of the falts containing them, as any difference obferved may be owing in part to a difference in the quantity of acid to which they are united, and in part to their different proportion of oxygen.

More oxygen feems to produce more difperfion in the muriate of iron;

A ftriking inftance of the latter is manifeft, from a comparifon of fulphur with the fulphuric acid; for, while the former appears to exceed the metallic oxides in difperfive power, the latter is inferior even to water.

but lefs in the oxidation of fulphur.

As I have likewife, at various times, made many experiments on difperfion by means of wedges, in a manner nearly fimilar to that employed by Mr. Dollond, Dr. Blair, and others, I have

The order of various bodies as to their difperfive powers;

I have endeavoured to reduce the ſeveral ſubſtances thus examined to one table; but, as the limits of colour are in few inſtances ſufficiently well defined for accurate menſuration, I have not attempted to add any numerical eſtimate of their powers, but have merely aſcertained the order in which thay ſucceed each other; and, in the following table, have arranged them according to the exceſs of their effect on violet above red light, at a given angle of deviation.

TABLE III.

tabulated with their refractive powers annexed.

Order of diſperſive Powers.	Refr. Power.	Order of diſperſive Powers.	Refr. Power.
Sulphur - -	2,04	Amber - -	1,547
Glaſs of lead ($\frac{1}{7}$ ſand)	1,987	Diamond - -	2,44
Balſam of Tolu -	1,60	Alum - -	1,457
Oil of ſaſſafras -	1,536	Plate-glaſs, Dutch	1,517
Muriate of antimony		Ditto, Engliſh -	1,504
Guaiacum - -	1,596	Crown glaſs -	1,533
Oil of cloves -	1,535	Ruby (ſpinelle) -	1,812
Flint-glaſs - -	1,586	Water - -	1,336
Colophony - -	1,543	Sulphuric acid -	1,435
Canada balſam -	1,528	Alcohol - -	1,37
Oil of amber -	1,505	Sulphate of barytes	1,646
Jargon - -	1,95	Selenite - -	1,525
Oil of turpentine -	1,47	Rock cryſtal - -	1,547
Copal - -	1,535	Sulphate of potaſh -	1,495
Balſam of capivi -	1,507	White ſapphire -	1,768
Anime - -	1,535	Fluor ſpar - -	1,433
Iceland ſpar -	1,657		

Numerous achromatic combinations.

By compariſon of this table with the order of refractive powers, as contained in the firſt table, it will be ſeen how little correſpondence there is between them; and, accordingly, how numerous are the combinations by means of which a pencil of rays that paſſes through two media, may be made to deviate without diſperſion of its colours.

White light does not appear to be ſeparable by the priſm into *ſeven* nor into *three* colours.

I cannot conclude theſe obſervations on diſperſion, without remarking that the colours into which a beam of white light is ſeparable by refraction, appear to me to be neither 7, as they uſually are ſeen in the rainbow, nor reducible by any means (that I can find) to 3, as ſome perſons have conceived; but that, by employing a very narrow pencil of light, 4 primary diviſions of the priſmatic ſpectrum may be ſeen, with a degree of diſtinctneſs that, I believe, has not been deſcribed nor obſerved before.

If a beam of day-light be admitted into a dark room by a crevice $\frac{1}{20}$ of an inch broad, and received by the eye at the distance of 10 or 12 feet, through a prism of flint-glass, *free from veins*, held near the eye, the beam is seen to be separated into the four following colours only, red, yellowish green, blue, and violet; in the proportions represented in Fig. 3. Experiments by which the colours are shewn to be four.

The line A that bounds the red side of the spectrum is somewhat confused, which seems in part owing to want of power in the eye to converge red light. The line B, between red and green, in a certain position of the prism, is perfectly distinct; so also are D and E, the two limits of violet. But C, the limit of green and blue, is not so clearly marked as the rest; and there are also, on each side of this limit, other distinct dark lines, *f* and *g*, either of which, in an imperfect experiment, might be mistaken for the boundary of these colours. Remarkable facts relating to the spectrum.

The position of the prism in which the colours are most clearly divided, is when the incident light makes about equal angles with two of its sides. I then found that the spaces AB, BC, CD, DE, occupied by them, were nearly as the numbers 16, 23, 36, 25. Spaces occupied by the colours;

Since the proportions of these colours to each other have been supposed by Dr. Blair to vary according to the medium by which they are produced, I have compared with this appearance, the coloured images caused by prismatic vessels containing substances supposed by him to differ most in this respect, such as strong but colourless nitric acid, rectified oil of turpenpentine, very pale oil of sassafras, and Canada balsam, also nearly colourless. With each of these, I have found the same arrangement of these four colours, and, in similar positions of the prisms, as nearly as I could judge, the same proportions of them. nearly the same in various bodies;

But, when the inclination of any prism is altered so as to increase the dispersion of the colours, the proportions of them to each other are then also changed, so that the spaces AC and CE, instead of being as before 39 and 61, may be found altered as far as 42 and 58.* but is changed with the inclination.

By

* Although what I have above described comprises the whole of the prismatic spectrum that can be rendered visible, there also pass on each side of it other rays, whereof the eye is not sensible. From Dr. Herschel's experiments (Phil. Tranf. for 1800) we learn, that on Invisible heat making rays on the confine of red;

Curious effects of the refraction of candle light;

By candle-light, a different ſet of appearances may be diſtinguiſhed. When a very narrow line of the blue light at the lower part of the flame is examined alone, in the ſame manner, through a priſm, the ſpectrum, inſtead of appearing a ſeries of lights of different hues contiguous, may be ſeen divided into five images, at a diſtance from each other. The 1ſt is broad red, terminated by a bright line of yellow; the 2d and 3d are both green; the 4th and 5th are blue, the laſt of which appears to correſpond with the diviſion of blue and violet in the ſolar ſpectrum, or the line D of Fig. 3.

and the light of electricity.

When the object viewed is a blue line of electric light, I have found the ſpectrum to be alſo ſeparated into ſeveral images; but the phenomena are ſomewhat different from the preceding. It is, however, needleſs to deſcribe minutely, appearances which vary according to the brilliancy of the light, and which I cannot undertake to explain.

on one ſide there are inviſible rays occaſioning heat, that are leſs refrangible than red light; and on the other I have myſelf obſerved, (and the ſame remark has been made by Mr. Ritter,) that there are likewiſe inviſible rays of another kind, that are more refracted than the violet. It is by their chemical effects alone that the exiſtence of theſe can be diſcovered; and, by far the moſt delicate teſt of their preſence is the white muriate of ſilver.

and diſoxygenating rays on the confine of violet.

To Scheele, among many valuable diſcoveries, we are indebted for having firſt duly diſtinguiſhed between radiant heat and light; (*Traité de l'Air et du Feu*, § 56, 57;) and to him alſo we owe the obſervation, that when muriate of ſilver is expoſed to the common priſmatic ſprectrum, it is blackened more in the violet than in any other kind of light. (§ 66.) In repeating this experiment, I found that the blackneſs extended not only through the ſpace occupied by the violet, but to an equal degree, and to about an equal diſtance, beyond the viſible ſpectrum; and that, by narrowing the pencil of light received on the priſm, the diſcoloration may be made to fall almoſt entirely beyond the violet.

It would appear therefore, that this and other effects uſually attributed to light, are not in fact owing to any of the rays uſually perceived, but to inviſible rays that accompany them; and that, if we include two kinds that are inviſible, we may diſtinguiſh, upon the whole, ſix ſpecies of rays into which a ſun-beam is diviſible by refraction.

VI. An

VI.

An Account of Dr. Young's *Harmonic Sliders. From his Paper in the Journals of the Royal Institution, p.* 261.

THE combination of undulations, however cautiously the world may adopt its application to the explanation of optical phenomena, is of acknowledged utility in illustrating the phenomena of musical consonances and dissonances, and of undeniable importance in accounting for many of the phenomena of the tides. Each tide is an undulation on a large scale; and, supposing the general form of the ocean, in consequence of the attraction of a distant body, to coincide with that of an oblong spheroid, as it is found by calculation to do, the section of the surface of each tide, if conceived to be unbent from the circular form and extended on a plane, would form the harmonic curve. (Young's Syllabus, IV. 151. 155.) It is remarkable that the motions of the particles of the air in sound have been generally supposed in theory to correspond with the ordinates of this same curve, and that there is also experimental reason to believe, that the purest and most homogeneous sounds do in fact agree very nearly with the law of this curve. It is therefore by far the most natural as well as the most convenient to be assumed, as representing the state of an undulation in general; and the name of these harmonic sliders is very properly deduced from the harmonic curve.

Utility of the doctrine of combined undulations in explaining cotemporary sounds; and the tides.

The harmonic sliders

By means of this instrument, the process of nature, in the combinations of motion which take place in various cases of the junction of undulations, is rendered visible and intelligible, with great ease, in the most complicated cases. It is unnecessary to explain here, how accurately both the situations and motions of the particles of air, in sound, may be represented by the ordinates of the curve at different points: it is sufficient to consider them as merely indicating the height of the water constituting a tide, or a wave of any kind, which exists at once in its whole extent, and of which each point passes also in succession through any given place of observation. We have then to examine what will be the effect of two tides, produced by different causes, when united. In order to represent this effect, we must add to the elevations or depressions in consequence of the first tide, the elevations or depressions

visibly exhibit the process of nature in the junction of undulations.

Explanation applied to tides.

preſſions in conſequence of the ſecond, and ſubtract them when they counteract the effect of the firſt: or we may add the whole height of the ſecond above any given point or line, and then ſubtract, from all the ſums, the diſtance of the point aſſumed below the medium.

Exhibition of a ſimple tide, To do this mechanically is the object of the harmonic ſliders. The ſurface of the firſt tide is repreſented by the curvilinear termination of a ſingle board, Plate VI. Fig. 1. The ſecond tide is alſo repreſented by the termination of another ſurface; but, in order that the height at each point may be added to the height of the firſt tide, the ſurface is cut tranſverſely into a number of ſeparate pieces or ſliders, which are confined within a groove or frame, and tightened by a ſcrew, Fig. 2. Their and of combinations which either increaſe, or modify, or deſtroy each other. lower ends are ſituated orginally in a right line; but, by looſening the ſcrew and moving the ſliders, they may be made to aſſume any other form: thus they may be applied to the ſurface repreſenting the firſt tide; and if the ſimilar parts of each correſpond, Fig. 3, the combination will repreſent a tide of twice the magnitude of the ſimple tides.

The more the correſponding parts are ſeparated, the weaker will be the joint effect, Fig. 4; and, when they are furtheſt removed, the whole tides, if equal, will be annihilated, Fig. 5. Thus, when the general tide of the ocean arrives by two different channels at the ſame port, at ſuch intervals of time that the high water of one would happen at the ſame inſtant with the low water of the other, the whole effect is deſtroyed, ex- The principle is applicable to a variety of caſes, cept ſo far as the partial tides differ in magnitude. The principle being once underſtood, it may eaſily be applied to a multiplicity of caſes: for inſtance, where the undulations differ in their dimenſions with regard to extent. Thus, the ſeries of ſliders being extended to three or four alternations, the effect undulations differing in extent. of combining undulations in the ratio of 2 to 1 of 3 to 1, of 2 to 3, of 3 to 4, may be aſcertained, by making a fixed ſurface, terminating in a ſeries of curves, that bear to thoſe of the ſliding ſurface the ratio required: and, by making them differ but The beat of an imperfect uniſon. ſlightly, the phenomenon of the beating of an imperfect uniſon in muſic may be imitated, where the joint undulation becomes alternately redoubled and evaneſcent. In Fig. 6, the proportion is that of 17 to 18, and the curvilinear outline repreſents the progreſs of the joint ſound from the greateſt degree of intenſity to the leaſt, and a little beyond it.

VII. *Obſervations*

VII.

Obſervations on the Appearances produced by the Colliſion of Steel with Hard Bodies. By Mr. DAVY, *Lecturer on Chemiſtry at the Royal Inſtitution* *.

I. MR. HAWKSBEE long ago ſhewed, that no ſparks could be produced by the colliſion of flint and ſteel † in the exhauſted receiver of an air pump, and that in this caſe a faint light only was perceived. And, ſince his time, the ſame obſervation has been very often made. Hawkſbee's exp. of flint and ſteel in vacuo.

The developement of the theory of combuſtion has clearly ſhown that the vivid ſparks obtained from ſteel in the atmoſphere, are owing to the combination of the ſmall abraded and heated metallic particles with oxigen. But it has been a matter of doubt whether, in the experiment made in vacuo, the faint luminous appearance is owing wholly to the light produced by the fracture and abraſion of the parts of the flint, or only partly to this cauſe, and partly to the ignition of the minute filaments ſeparated from the ſteel. Sparks thus produced in air, are owing to combuſtion of the ſteel.

II. I have often found, that when a fine and thin flint, which may be eaſily broken, is uſed for the colliſion in vacuo, the light is much more vivid than when a thick and ſtrong one is employed: and with a ſtrong flint, but juſt ſharp enough to give ſparks with ſteel in the atmoſphere, it is ſeldom that any light at all is produced in the exhauſted receiver. Theſe facts ſeem to ſhew that the abraded particles of ſteel are not rendered at all luminous by colliſion, except in conſequence of combuſtion; and the opinion is almoſt fully proved by the following experiment, which was made in the courſe of a lecture on the properties of light, in the theatre of the Royal Inſtitution, and which has been ſince often repeated. Light in vacuo is moſt vivid with a thin ſharp flint. The colliſion does not make the ſteel red hot.

III. A thin piece of iron pyrites ‡ (ſulphuret of iron) was inſerted in a gunlock in the place of the flint. It gave by Pyrites inſtead of flint, gave no light in vacuo.

* Journal of the R. I. 264.

† Philoſophical Tranſactions, Vol. XXIV. p. 2165.

‡ The etymology of the name of this ſubſtance ſhews that its property of giving fire by colliſion was very anciently known. It was uſed in the old gunlocks, with the revolving wheel, for inflaming the priming.

colliſion

colliſion in the atmoſphere very vivid ſparks; which were chiefly white, from the combuſtion of the particles of the ſteel; but ſometimes mixed with a few red ſparks from the combuſtion of the particles of the pyrites. The gunlock was introduced under the receiver of an air pump, and the exhauſtion was made till the mercury in the ſhort gage ſtood at about $\frac{6}{10}$ of an inch. The lock was then ſnapped, but no light whatever was perceived; and the phenomenon was uniform, every precaution being taken to render the room dark, and to preſerve the apparatus in order.

Queſtion. Can ſteel burn in air if not firſt white hot?

IV. It is well known that in common caſes the fineſt ſteel wire does not burn with a white light or ſparks in the atmoſphere, unleſs it have been previouſly heated to a degree much above that of the red heat; it conſequently at firſt view appears extraordinary, that the particles ſeparated from the gunlock ſhould be heated ſo as to burn vividly in air, and yet not ſo as to appear ignited in vacuo; for it is not eaſy to conceive that they emit light, which from the minuteneſs of their volume * cannot be perceived; or to ſuppoſe that the opacity of the metallic ſubſtances ſhould hinder light generated at their points of contact from being viſible. I had formerly ſuppoſed, in reaſoning upon the phenomenon of the colliſion of flint and ſteel, that † heat and light might in common inſtances be only accidentally coexiſtent; and that in certain caſes very high temperatures might be produced without cauſing the appearance of light. At preſent however I am inclined to believe, that the phenomena may be adequately accounted for upon principles that coincide with the common facts relating to the production and communication of heat.

If it cannot, why is not this white heat ſeen in vacuo?

Explanation. Oxidation at temperatures far below ignition, may develope more heat than can be conducted off, and may produce vivid combuſtion;

Mr. Stodart ‡ has ſhewn, that when ſteel is gradually heated it begins to change colour at about 430° Fahrenheit. And this change of colour is occaſioned by its combination with oxigen, and, as there is every reaſon to believe, muſt be connected with the evolution of heat. At about 600°, a temperature much below that of ignition, it oxidates rapidly, and becomes covered with a bluiſh grey coating. And though in theſe caſes of oxidation the heat evolved at the ſurface of

* Or the ſhort time of its emiſſion.—N.

† Nicholſon's Journal, 4to, Vol. III. p. 517.

‡ Nicholſon's Journal, 4to, Vol. IV. p. 130.

the

the metal is not ſufficient to raiſe the temperature of ſteel wire, or a ſteel plate, ſo as to cauſe it to enter into the vivid combuſtion, yet in acting upon ſuch a minute filament as that ſtruck off in the gunlock, it may be ſufficient to keep up the proceſs of oxidation till it becomes ſo vivid as to occaſion the ſtrongeſt heat and light. Beſides, the ſurface of this filament is very great as compared with its bulk, and the oxide produced upon it is leſs likely to form a coat which might defend the interior parts from the action of the air *.

In caſe the ſurface be very great in proportion to the maſs.

It would not be difficult to find many analogous inſtances, in which the progreſs of oxidation is dependent upon the maſs of the combuſtible body, or rather upon the relation of this maſs to ſurface; thus, a very thin and ſmall bit of phoſphorus will inflame ſpontaneouſly, and burn with the vivid light when wrapped up in filaments of fine cotton; whilſt a thicker and larger piece will only ſhine with the feeble blue light: and though a large maſs of zinc may be melted in the atmoſphere without inflaming, a ſmall and thin ſhaving will burn vividly long before it is heated to the temperature of fuſion.

Other inſtances. Phoſphorus and zinc readily take fire in ſmall maſſes.

V. In conſidering the general phenomena of the production of heat and light, by mechanical means, it is difficult to conceive that any conſiderable increaſe of temperature can be produced on a metallic ſurface by a ſingle colliſion; for the conducting power of the metals is ſuch as would ſpeedily cauſe the heat to be communicated to the contiguous parts; and even in the caſe of the abraſion of minute particles, though the time required for their ſeparation from the maſs is to us imperceptible, yet it muſt be ſufficient to enable them to give out to it a portion of their heat.

Not probable that much heat ſhould be produced by a ſingle colliſion.

The bodies that become luminous by being ſtruck or rubbed together in vacuo, or in gaſes that do not contain oxigen, or under water, ſuch as fluate and carbonate of lime, ſiliceous ſtones, glaſs, ſugar, and many of the compound ſalts, are both electrics *per ſe*, and phoſphoreſcent ſubſtances; ſo that the flaſhes they produce are moſt likely occaſioned, partly by

Luminous appearances, from electricity or phoſphoreſcence, by friction.

* In turning very fine work of ſteel in the lathe, ſo as to afford ſhavings or threads much thinner than one-thouſandth of an inch, I found that this metallic wool very readily caught fire at a candle, and burned throughout in quantities of a cubic inch or more: But it was ſcarcely ſo much oxided as to have loſt its flexibility after this combuſtion.—N.

the

the electricity excited on their surfaces by the friction, and partly by their phosphorescence, which is generally occasioned by moderate degrees of heat. It is not however improbable that in some cases, by the collision of very hard stony bodies, which are bad conductors of heat, there may be an actual ignition of abraded particles; and the supposition is countenanced by various facts. Mr. T. Wedgwood found that a piece of window glass, when brought in contact with a revolving wheel of grit, became red hot at its point of friction, and gave off luminous particles which were capable of inflaming gunpowder and hidrogen gas *. And we are informed by a late voyager †, that the natives of Oonalashka light their fires by striking together two pieces of quartz, their surfaces being previously rubbed with native sulphur, over dry grass.

Cases of actual ignition:

VIII.

Description of a Blow-pipe by Alcohol, having a safety Valve, with other Advantages. Constructed by Mr. BENJAMIN HOOKE, *Fleet Street.*

To Mr. NICHOLSON.

SIR,

Blow-pipe with a safety valve and only one flame.

I HAVE taken the liberty of troubling you with a drawing and description of the *blow-pipe by alcohol* as I make it, which perhaps possesses the following advantages over that described in your number of September last; *viz.* Being furnished with a safety valve, to prevent accidents; having only one lamp (the wick of which being pretty large, answers both for heating the alcohol and for affording a strong blast when drawn through it); and I think superiority as to form and appearance.

Should you esteem it worthy a place in your valuable Journal, the insertion will oblige,

SIR,

Your very humble servant,

BENJAMIN HOOKE.

159, *Fleet Street,*
Nov. 20, 1802.

* Phil. Trans. 1792, p. 45.

† Sauer's Account of Billings's Expedition to the northern parts of Russia, p. 159.

A is

A is a hollow ſphere * for containing alcohol, reſting upon a ſhoulder in the ring O, Plate V. Deſcription.

B is a bent tube with a jet at the end, to convey the alcohol in the ſtate of vapour into the flame at Q, this tube is continued in the inſide up to C, which admits of A being filled nearly, without any alcohol running over.

D is a ſafety valve, the preſſure of which is determined at pleaſure, by ſcrewing higher or lower on the pillar E, the two milled nuts F and G carrying the ſteel arm H, which reſts on the valve.

I is an opening for putting in the alcohol.

K is the lamp, which adjuſts to different diſtances from A, by ſliding up or down the two pillars L. The diſtance of the flame Q from the jet, is regulated by the pipe which holds the wick being a little removed from the centre of the braſs piece M, and of courſe revolving in a circle.

N the mahogany ſtand.

IX.

Deſcription of a Joint applied to Tubes uſed for conveying Steam under conſiderable Preſſure. W. N.

THE valuable communication of my correſpondent N. N. in the laſt number, has fixed my recollection upon ſeveral parts of my hydraulic and pneumatic apparatus, in which, though they may be called bagatelles as far as relates to the magnitude of the inventive powers required to make them, yet their utility may be ſuch as in many inſtances to prevent leſs eligible contrivances from being adopted. Recollection of ſome apparatus.

In the firſt place, I find among ſome braſs work the ingenious joint, Plate II. Fig. 2, lately contrived by himſelf, and before him by Mr. Webſter, but in this caſe certainly made many years ago by ſome unknown mechanic. My piece, which is for ſpouting fluids, is prevented from being driven aſunder by a wire that paſſes through its axis, and has a ſmall nut and waſher faced with leather. Early invention of the joint for pneumatic apparatus.

* If the bottom is made flat inſtead of ſpherical, the action of the flame will then be greater.

The

Another joint for ftrong fteam.

The apparatus of which I have given a drawing and fection in Plate VII. was made for conveying fteam from a boiler to the fteam engine, and for many other philofophical purpofes, in which this agent is very ufeful, though hitherto not much applied to objects of this nature. As I have found it very commodious, I fhould not be difpofed to make any remarks on the contrivance itfelf, if its refemblance to fo many things that I have feen, both in organization and effect, did not make it proper to fay, that I fhould have let it pafs unpublifhed (among the many arrangements, whether old or new, which every practical mechanic finds himfelf induced to adopt in his operations) had not the confideration of utility made me fuppofe it might prove acceptable.

Delineation. A wide conical cock with joints in the tubes, preffed together with a fcrew and a difh fpring.

In Fig. 1, 2, and 3, the fame letters of the alphabet denote the fame things; Fig 1 exhibiting a view, Fig. 2 a fection, and Fig. 3 the plan of fome of the parts. The letter *e* fhews part of an upright tube conveying fteam from the boiler into the body of the cock *c* by its hollow cone *m*, which is open beneath and clofed above, and has a fide aperture *n* communicating, when duly placed, with the pipe *h i*. The cone is preffed into its place by the cap *b* fcrewed on the body of the cock *c* at *r*. The cap does not prefs immediately upon the upper furface or fhoulder of the cone, becaufe the motion would in that cafe be hard and uncertain; but it acts by the intervention of a fpring *o* in the fhape of a difh or flightly concave place, which keeps it to its bearing with any required degree of preffure accordingly as the cap is more or lefs fcrewed down, and always with a moft fmooth and pleafant action without the leaft jerk or obftacle. The cock is turned by the wooden handle or lever *a*, and is very fecure on account of the large furface of the convex and concave cones which remains imperforated.

The pipe between *h* and *i* is connected by a joint, in which *g* is a loofe cap milled on the outfide, and having a concave fcrew on its inner furface. It is fcrewed upon the fixed piece *f*, which is turned to fit it: In this laft piece is a cylindric concavity furrounding the orifice of the pipe *h* (fee *f*, Fig. 3), into which the piece *p*, at the extremity of the pipe *i*, loofely fits. Upon the circular face of *i* are turned three or four grooves, and the like on the correfpondent face of *f*. Thefe are of ufe to fecure a fmall piece of tallowed linen cloth put between them

them when applied together. Laſtly, there is a diſh ſpring *o* between *p* and *g*. When *g* (which ſlides looſely upon the pipe *i*) is ſcrewed upon *f*, it preſſes *p* by the intervention of the ſtiff diſh ſpring againſt the interior of *f*, and makes a ſecure junction; but the pipe *i*, repreſented in Fig. 1 as pendant, or in a vertical poſition, may be moved round in the ſame plane to any elevation or depreſſion whatever.

The cap *d* being ſcrewed on the body of the cock at *t*, conſtitutes a joint of the ſame kind, by means of which the pipe *i* may be moved into any horizontal poſition: Conſequently, by theſe two motions, that pipe may be placed in every poſſible ſituation.

If we ſuppoſe the pipe *i* that proceeds immediately from the cock, to be of conſiderable length, as for convenience it is required to be, its poſſible changes of poſition will not be ſufficient to adapt it to the uſual operative purpoſes. It may therefore, in ſeveral caſes, be convenient to apply two other ſimilar joints *k* and *l* at right angles with each other, by means of which it will be immediately ſeen, that the exterior pipe *w* may be placed in any required direction, not merely with regard to a point immediately above the boiler, but with regard to another moveable point at an equal diſtance from the fire.

The diſh ſpring generally recommended.

I ſhall conclude this paper by obſerving, that the bended waſher or diſh ſpring is uſed in the vice and ſome other mechanical tools, but that it far from being as much uſed as it ought to be. For there is ſcarcely any action of pinching, preſſing, or binding, in which it would not be advantageous, and would, in many caſes, prevent ſerious miſchief and inconveniences.

X.

Memoir on Achromatic Glasses adapted to the Measure of Angles, and the Advantages that may be derived from double Refraction for the precise Measurement of small Angles: by ALEXIS ROCHON, *Member of the National Institute, and Director of the Naval Observatory at Brest* *.

The position of places determined by angles.

WHEN we would determine the position of places, either on the earth or in the heavens, our conclusions are always founded on angles obtained by observation; and of course the certainty of our conclusions must depend on the precision with which we can measure these angles †. Euler was the first who attempted to correct the aberrations of refrangibility, by employing substances of different refractive powers. Maupertius procured Euler's object glass, consisting of glass and water, to be made at Paris: but this we now know could not succeed, from the ratios the refractions and dispersions of common glass and water bear to each other.

Euler said to have first attempted to correct the aberrations of refrangibility by a lens of glass and water;

and afterward Dollond by flint glass and crown glass;

In 1755, Mr. Dollond laid before the public his achromatic object glasses, composed of flint glass and crown glass. He says in his paper in the Philosophical Transactions, that he destroyed the aberrations of refrangibility with tolerable ease; but to destroy that of sphericity at the same time, was an object much more difficult to surmount. For this Dollond obtained a patent, the exclusive privilege of which was attacked by Mr. Watkins of Charing Cross, on the ground that the discovery had been made long before by a gentleman of the name of Hall, who however had not rendered it public. It is said that Mr. Hall gave orders for his lenses of flint glass to an optician in one part of London, and for those of crown glass to another in a distant part, that his secret might not be discovered; and that both of them happened to employ the same workman to make them, who suspecting some mystery, as he

an invention which he is said to have stolen from Hall.

* Abridged from the Journal de Physique, Fructidor, year 9.—C.

† The history of the invention of achromatic glasses is given very erroneously in this and most foreign memoirs. I purpose soon to give a commentary on this subject.—N.

knew

knew they were for the ſame perſon, ſhewed them to Dollond, for whom likewiſe he worked, and who thus learned what he afterwards publiſhed as his own diſcovery.

Be it as it may, I ſhall with difficulty be perſuaded, that Euler was not the firſt who thought of correcting the aberrations of refrangibility, as Newton invented the means of correcting the aberrations of ſphericity, by compoſing an object glaſs of glaſs and water. Was it not the illuſtrious Euler, who, reflecting on the ſtructure of the eye, ſuſpected this organ was compoſed of different mediums for the purpoſe of deſtroying the confuſion produced by the decompoſition of light when it traverſes a ſingle medium? And did he not publiſh in the Memoirs of Berlin and of Peterſburg, that ſublime idea, which accorded with the ſyſtem of perfection he aſcribed to all the works of the Supreme? It is true, the inutility of this correction of the aberrations for the ſphere of diſtinct viſion, from the ſhortneſs of the form of the eye, may be objected to Euler's hypotheſis: but this makes nothing againſt his claim to the diſcovery, to which he was led by it. Neither had this happy idea that ſucceſs at the time, which might have been hoped from it; partly becauſe the theory of this great geometrician was founded on laws of refraction purely hypothetical; partly becauſe it was repugnant to the aſſertion of Newton, that, when light traverſes ſeveral mediums of different natures, ſo that the emergent rays are parallel to the incident, the light is not decompoſed. It is well known, that the learned Klingenſtiern, in 1755, expreſſed his doubts of the laws of refraction advanced by Newton: and that theſe doubts were confirmed in 1759, by Dollond's experiments on crown and flint glaſs, which firſt exhibited to the learned colourleſs priſms.

But Euler the firſt diſcoverer of the principle, to which he was led from reflecting on the ſtructure of the eye.

Though the correction he aſcribed to the ſtructure of the eye not neceſſary in this organ.

His idea neglected, becauſe his theory was hypothetical, and contradictory to an aſſertion of Newton's.

Klingenſtiern expreſſed his doubts of Newton's laws of refraction; and Dollond confirmed theſe doubts.

It was in the beginning of 1774, that I read to the academy a memoir, in which I propoſed to improve achromatic object glaſſes by the interpoſition of a fluid between the two lenſes of crown and flint glaſs which compoſe them. Borda, le Gentil, and Caſſini, were appointed a committee to repeat my experiments. They obſerve in their report, that, "in an object glaſs of three lenſes, if there be a difference of a thouſandth part of a line between the curvature of the centre and that of the edges of each ſurface, a ſenſible imperfection in the image of the object will be the conſequence. But the mere heat of the workman's hand, in giving the glaſs the finiſhing poliſh, is capable of dilating

Rochon propoſes to improve Dollond's compounded object glaſs, by introducing a fluid between the pieces.

Report of Borda and others on this ſubject.

lating it ſenſibly, ſo that the difficulty of not producing ſome inequalities of curvature in large glaſſes muſt be very great. Perhaps it is impoſſible to avoid this imperfection, but it may be corrected: and our colleague affirms, that the effect of the imperfections of the four interior ſurfaces of the three glaſſes may be diminiſhed conſiderably, by the introduction of a diaphanous fluid between them. Experiments alone can decide the fact. We took therefore an achromatic teleſcope three feet long, the aperture of which was about three inches. The two lenſes compoſing the object glaſs, being about half an inch diſtant from each other, we introduced between them a thin Bohemian glaſs not wrought. In this ſtate it is obvious the teleſcope muſt be very bad; and accordingly, on directing it to ſome writing, we were obliged to bring this within eleven yards and half of it, to diſtinguiſh the letters. Having then poured pure water between the object glaſſes, ſo as to fill the intervals between them, we found the letters not more difficult to be diſtinguiſhed at the diſtance of ſixty-two yards. We might have purſued our experiments further, but this was ſufficient to eſtabliſh the principle, which was all we had in view."

Their experiments on it, which are in favour of the principle.

At this time I was ordered to Breſt, which put a ſtop to my purſuit. In my memoir I had advanced, that the confuſion ariſing from the irregularity of the glaſs of a teleſcope, was to the confuſion which takes place when the glaſs is immerſed in a fluid, as the refraction of the glaſs is to the difference of refraction between the glaſs and the fluid: conſequently, if the fluid have the ſame refractive power as the glaſs, no alteration in the diſtinctneſs of the object will be perceptible; but if the glaſs be immerſed in water, the confuſion will be about a ſeventh only of what would have taken place, had the object been ſeen directly through the irregular glaſs. This calculation, it is true, ſuppoſes the glaſs to be in perfect contact with the fluid; but there are obviouſly many cauſes by which this contact may be influenced. It may have been owing to ſome ſuch cauſe, that oil, the refractive power of which differs leſs from that of glaſs, was found by experiment not to anſwer ſo well as water; for writing, which was legible at the diſtance of 130 feet when water was employed, could be read only at eighty-eight feet when oil was interpoſed between the glaſſes *.

To make a perfect object glaſs, the refractive power of the interpoſed fluid ſhould be the ſame with that of the glaſs.

Water reduces the confuſion to one ſeventh;

but oil, though its power is nearer that of glaſs, does not reduce it ſo much.

* On this ſubject, ſee Philoſ. Journal, 4to, III. 000.

By the Regifters of the Academy of Sciences, January the 6th, 1788, it appears, that Grateloup adopted the fame method. He did not make ufe of a fluid interpofed between the glaffes, however, but of maftic. This fubftance had long been in ufe among jewellers, for cementing together ftones, which it did in fuch a way, that it was fcarcely poffible to diftinguifh the two fo united from a fingle ftone; and hence Grateloup conceived, it would render his object glaffes as it were one folid piece, without being liable to evaporation like a fluid, while it corrected the errors of their interior furfaces. Glaffes cemented together with maftic, however, do not anfwer, at leaft for fea voyages, as change of temperature, and the fea air, affect the maftic very much. On this account I have preferred refin, and even turpentine the moft fluid and tranfparent I could get. Chemifts perhaps may find fubftances ftill preferable to thefe, and it is an object certainly worthy their attention. Dr. Blair tried a great number of folutions of metals and femimetals, and he fays, that certain falts, particularly fal ammoniac diffolved in water, give it a confiderable power of difperfion. The oxigenated muriatic acid, too, poffeffes this quality in a great degree; but butter of antimony has a ftill greater effect, for one prifm of this is equal to three of crown glafs of fimilar dimenfions. Dr. Blair conftructed an object glafs with crown glafs and butter of antimony, but he obferved, that it occafioned irradiations, which led him to prefer oil of turpentine or other effential oils *.

Grateloup ufed maftic between the glaffes.

But change of temperature, and the fea air, injure maftic.

Refin therefore preferable, or turpentine.

Dr. Blair tried various fubftances; fome falts, and oxigenated muriatic acid, had a confiderable power of difperfion; and muriate of antimony had ftill more; but this occafioned irradiations, whence he preferred effential oils.

The ufe of achromatic glaffes applied to graduate circles or fegments of circles for meafuring angles, and the defects to which inftruments of this kind are liable, may be paffed over as not to the prefent purpofe, which is the beft mode of meafuring very fmall angles.

Buffon, who had paid fome attention to the formation of rock cryftals, finding no indication of a double refraction in the experiments which I made on that of Madagafcar, in 1770, thought this tranfparent quarter to be of a different nature from the other cryftals; but before he made up his mind on the fubject, he defired me to examine it afrefh with a view to this property.

Buffon fufpected the double refraction of the rock cryftal of Madagafcar might have been overlooked.

* An ample abridgement of Dr. Blair's paper is given, Phil. Journal, 4to, I. 1.

Beccaria published a curious work on the refraction of cryſtals.

It may not be improper here to give a brief abſtract of a very curious work, which Beccaria publiſhed in Italian about this time, as I learned from it the direction in which it was neceſſary to cut my priſms, to be certain of obtaining the effects I ſought. Huygens, ſays Beccaria, who treats very fully on the double refraction of Iceland cryſtal, adds, that rock cryſtal poſſeſſes the ſame property, but in a leſs ſenſible degree. I ſaw that the refraction which Huygens calls irregular, bears a conſtant relation in the Iceland cryſtal to its internal ſtructure; for the effect of this refraction is to divert the rays in the direction of the ſaliant angles, which are equal in the whole piece, and in each of its component parts. From inſpecting the figure of rock cryſtal, I inferred its internal ſtructure. In fact, I conſidered it as ſimilar to artificial cryſtallizations. formed by the aggretation of little laminæ parallel to the faces of the cryſtals. Suſpecting Huygens of inaccuracy in aſſerting, that priſms of rock cryſtal had always a double refraction, whatever their ſections were, I cut a piece of rock cryſtal, of a very irregular figure, in the direction of its axis, and ſo as o divide two of its oppoſite faces into two equal parts. I cut another likewiſe in the direction of its axis, but ſo that the ſection paſſed through the ſummits of two oppoſite angles. A third afforded me triangular priſms, one of the faces of which was one of the faces of the cryſtal itſelf, while each of the other two faces terminated on one ſide at one of the contiguous angles, and on the other at the axis of the cryſtal. A fourth was divided into equilateral priſms, two of the faces of which were equally inclined to the axis, and the third was parallel to a plane continued through the axis. From the obſervations made with theſe priſms, I obtained a very ſimple law, of which Newton ſays nothing, and the contrary of which is advanced by Huygens. 1. That the double refraction does not ſubſiſt in all the different priſms that may be cut from rock cryſtal, 2. That the ray of light which traverſes rock cryſtal in a place perpendicular to the axis undergoes two refractions, is divided into two, and gives two images, if not completely yet ſenſibly diſtinct. 3. That this diſtinctneſs of the two images diminiſhes in proportion as the direction of the ray approaches to that of the axis of the cryſtal. 4. That the double refraction and diſperſion of the two images ceaſe entirely, when the direction of the ray is parallel to the axis, or nearly parallel to it.

Huygens miſtaken in calling the refraction irregular; as it uniformly depends on the internal ſtructure of the cryſtal.

Beccaria cut rock cryſtal in different directions;

whence he obtained certain laws of their refraction.

it. Hence it follows, that in cutting a lens of rock cryſtal for optical purpoſes, we muſt take pieces parallel to the baſe of the cryſtal, ſo that the axis of the lens coincides with that of the cryſtal, or is at leaſt parallel to it. Thus far Beccaria.

The refraction of the cryſtal is ſingle in the direction of its axis.

When we would apply to practice this properly in rock cryſtal of doubling the images of objects, the colours that ariſe from the priſmatic figure neceſſary to be given to it muſt be corrected by a glaſs priſm, or the images will not appear well defined. Theſe two priſms united together may be called a *double refracting achromatic medium,* but in this ſtate it can ſerve only to meaſure the ſmall angle given by the double refraction: yet it is eaſy to conceive, that on applying two double refracting mediums one on the other, we may vary the effect of the double refraction at pleaſure by a circular movement, as in the inſtrument for meaſuring colours which I laid before the Academy. Thus this double refraction, which has been deemed detrimental to the conſtruction of optical inſtruments, is in fact advantageous for the menſuration of ſmall angles, as I convinced the Academy, February the 25th, 1777, by an inſtrument conſtructed on this principle, which gave the meaſure of ſmall angles with a degree of preciſion, for which we could ſcarcely have hoped. This inſtrument, however, had the inconvenience of giving four images of one object, which occaſioned a conſiderable loſs of light; and the leſs light we have, the leſs the accuracy with which the point of contact can be obſerved.

A priſm of rock cryſtal muſt be corrected by one of glaſs:

the two forming a *double refracting achromatic medium.*

Inſtrument for meaſuring ſmall angles by means of two of theſe mediums.

Its defect.

Endeavouring to remedy this imperfection, I ſoon diſcovered a more ſimple conſtruction for the inſtrument, which I laid before the Academy the ſame year. In this only one double refracting achromatic medium is required, which is made to move along the inſide of any teleſcope in the axis of the object glaſs. The value of the double refraction is firſt to be determined by experiment, as in the former micrometer, the achromatic medium being placed againſt the object glaſs. It is then to be moved from the object glaſs toward the eye glaſs. The angle of the double refraction will now be what it was in the former ſituation, but the ſeparation of the images will be in the ratio that the diſtance of the refractive medium from the focus bears to the focal diſtance. Suppoſe for example the double refraction to be twenty-one minutes, and the focal diſtance of the object glaſs to be three metres: if the

This remedied by uſing one medium, made moveable along the axis of the teleſcope.

 achromatic

achromatic medium be brought nearer by two thirds of this diſtance, the double refraction will occaſion only a ſeparation of ſeven minutes between the images. Thus if we would meaſure the diameter of an object, we muſt move the achromatic medium toward the focus, till the two images of the object are ſeen preciſely in contact; when, having the angle of double refraction given, by previous experiment, the focal diſtance of the object glaſs, and the diſtance of the achromatic medium from the focus, the diameter will be given by the rule of proportion. It is obvious, that great care muſt be taken in determining the angle of double refraction at firſt with preciſion, as on this will depend the accuracy of all ſubſequent meaſures with the inſtrument.

Mode of uſing this micrometer.

Having thus improved the conſtruction of this micrometer, I found that the glaſs priſms, intended to correct the diſperſion of the priſm of rock cryſtal, left a refraction more and more perceptible in proportion as the double refracting medium was carried farther from the eye glaſs. I then availed myſelf of Beccaria's diſcovery, and cut my two priſms ſo that the firſt was in the direction of the double refraction, and the ſecond in that in which the double refraction is not perceptible: by theſe means I had a double refracting medium abſolutely exempt from colours and refractions. I did not ſtop here, for I was deſirous of extending the effect of the double refractions ſo as to meaſure the diameter of the ſun; and I accompliſhed this, which appeared to exceed the known power of the double refraction of rock cryſtal, which does not go beyond twenty minutes, when it is cut in the moſt advantageous priſmatic ſhape. For this I employed two equal priſms, cut in the direction moſt ſuitable to my purpoſe; and on placing them in oppoſite directions I found, that the double refraction was not perceptible; but on reverſing their directions the double refraction of each priſm was nearly doubled, ſo that I obtained two images ſeparated by an interval of forty minutes.

The inſtrument farther improved by uſing two priſms of rock cryſtal cut in oppoſite directions.

The effect of the double refraction doubled,

by means of two equal priſms.

I ought not to omit, that in this new conſtruction there are difficulties of execution not eaſy to ſurmount, which may have been one reaſon why theſe inſtruments, ſo uſeful to navigators and in certain very nice aſtronomical obſervations, have not been adopted. This induced me at length to adopt Euler's method. In the conſtruction of achromatic object glaſſes I found I could increaſe or diminiſh the abſolute effect of the

This inſtrument difficult to execute.

Hence the author was led to avail himſelf of Euler's method.

Effect of the

double

double refraction within certain limits, by means of the interval between the glasses of differing refracting powers: the separation of the images at the focus being so much the greater, as the interval is larger, when the flint glass is the first of the object glasses; and less, when it is the second.

double refraction increased by increasing the interval between the object glasses when the flint glass is first; the reverse when it is the second.

Conformably to these new principles I have had two telescopes with a double refracting medium constructed under my own inspection, which General Gantheaume will employ for determining the position of his ships, and to find whether he be approaching any he may meet with at sea *.

The uses of an instrument for measuring very small angles with precision are too well known for me to describe its advantages. The officers of the English navy are so fully aware of them, that they have used for some years Ramsden's eye glass micrometer, though this answers the end but imperfectly, because it does not give the measure of the angle, and because of the bad effect of the parallax produced by the decussation of the rays that enter the eye. This defect is more sensible in Ramsden's eye glass micrometer than in Bouguer's heliometer. The officers who have compared my instrument with Ramsden's, of which there were several on board the Spanish ships with our Brest fleet, agree that the celebrated English artist has very imperfectly accomplished the object he proposed; and Bouguer's heliometer could unquestionably be preferable for naval use, because it has a less sensible parallax, and gives the measure of small angles, so important for determining the distances of ships from their known dimensions.

Defects of Ramsden's eye glass micrometer.

Bouguer's heliometer preferable.

Table of the proportions which the magnitude of an object bears to its distance: calculated according to the rule, which is easily demonstrated, that, in every right angled triangle, the tangent is to the radius as the magnitude of an object is to its distance from the centre of the eye, when, under an angle known, the distance forms a right angled triangle with the object.

* The General has made an advantageous report of this instrument, in his account of the chace of the Swiftsure, which he captured. This instrument is so difficult to execute, that I know only one person, Citizen Narci, who is capable of giving rock crystal the prismatic form in the proper direction for obtaining the double refractions necessary to the goodness of the micrometer. He has made several, which have given me perfect satisfaction.

only one person yet found capable of making this micrometer.

EXAMPLE.

EXAMPLE.

General formula for calculating diſtances.

Suppoſe the angle be meaſured under which we perceive an object of the magnitude of one toiſe, placed vertically ſo as to repreſent the tangent, the radius of which is the diſtance from the eye; if this angle be of 30′ we ſhall have the following proportion: the tangent of 30′ is to the radius, as one toiſe is to the diſtance. By the tables of logarithms we ſhall have this diſtance by ſubtracting from the logarithm of the coſine the logarithm of the tangent of 30′, which is 7.940858: the logarithm reſulting from this ſubtraction is 2.059142; and the number anſwering to this logarithm is 114.6 nearly; conſequently the diſtance from the eye to the object, the magnitude of which is ſuppoſed to be one toiſe, is 114 toiſes ſix tenths; but in the uſe of the inſtrument fractions may be neglected.

It is proper to obſerve, that in meaſuring ſmall angles the triangle need not be perfectly rectangular: the object may ſenſibly vary from the perpendicular, without a perceptible difference in the angle.

QUESTION I.

Mode of determining by the micrometer the diſtance of an object of known magnitude.

The magnitude of an object being known, to determine its diſtance.

For this purpoſe the two images of the object produced by the priſms are to be brought into contact, by moving the index along the axis that carries them. This rule has two diviſions, one ſhewing the minutes and ſeconds, the other the proportion between the diameter of the object and the diſtance. Suppoſe the object I look at is a man: the mean height of a man with his hat on may be eſtimated at ſix feet. Having diſpoſed the priſms and teleſcope ſo that the two images are in one vertical line, I move the index till the feet of the upper image come into contact with the ſummit of the lower: if now the index point at the number 344, I conclude that the diſtance of the man from my eye is 344 times ſix feet, or 344 toiſes. The ſecond diviſion will ſhew, that the angle under which I ſee the man is 10′. By this method the meaſure of a diſtance may unqueſtionably be obtained with great preciſion, but there are many caſes, in which this great preciſion is not neceſſary.

Ships,

Ships, buildings in which certain rules little liable to variation are obferved, and the dimenfions of different animals, may ferve to give this diftance. By means of fuch objects I have often determined inacceffible diftances with truly furprifing celerity, by means of a portable telefcope with prifms, which requires no fupport for taking angles. When greater accuracy is required, objects of which the diameter is well afcertained muft be employed.

If lighthoufes had on them a crofs of fixed and known dimenfions, it would thus be of great fervice to navigators: for by the angle of the perpendicular piece, given by this micrometer, the diftance of the fhip from the lighthoufe would be afcertained; and by that of the horizontal piece would be known the pofition of the fhip with refpect to the coaft.

Croffes of known dimenfions on light-houfes would be of great utility to navigation.

QUESTION II.

The diftance of an object being given, to determine its magnitude.

This is precifely the reverfe of the preceding.

The magnitude of an object determined from the diftance known.

QUESTION III.

The magnitude and diftance of an object being unknown, to determine both.

Suppofe the object makes an angle of 40′ with my eye; as this angle will increafe in regular proportion as the diftance is diminifhed, I approach the object till the angle is 41′, a difference which will be fufficient in ordinary cafes. If I find the diftance between the two ftations, on meafuring it, to be a hundred toifes, I fhall obtain the whole diftance by multiplying this by the number of minutes of the larger angle, and dividing the product by the difference between the two angles, which will give in this inftance 4100 toifes. The diftance being thus obtained, the magnitude of the object will be found by dividing 4100 toifes by $83\frac{1}{2}$, the number anfwering to 41′; which will give the magnitude forty-nine toifes feven inches nearly.

Method of determining the magnitude and diftance, where both are unknown.

XI. *Obfervations*

XI.

Obſervations on the two lately diſcovered celeſtial Bodies. By WILLIAM HERSCHEL, *LL. D. F. R. S.*

The moving ſtar of Piazzi is very minute.

IN my early account of the moving ſtar diſcovered by Mr. Piazzi, I have already ſhewn that it is of a remarkably ſmall ſize, deviating much from that of all the primary planets *.

It was not my intention to reſt ſatisfied with an eſtimation of the diameter of this curious object, obtained by comparing it with the Georgian planet, and, having now been very ſucceſsful in the application of the lucid diſk micrometer, I ſhall relate the reſult of my inveſtigations.

Another diſcovered by Dr. Olbers.

But the very intereſting diſcovery of Dr. Olbers having introduced another moving ſtar to our knowledge, I have extended my reſearches to the magnitude, and phyſical conſtruction, of that alſo. Its very particular nature, which, from the obſervations I ſhall relate, appears to be rather cometary than planetary, will poſſibly throw alſo conſiderable light upon the circumſtances belonging to the other celeſtial body; and, by that means, enable us to form ſome judgment of the nature of both the two laſt diſcovered phenomena.

As the meaſures I have taken will oblige me to give a reſult which muſt appear extraordinary, it will be highly neceſſary to be particular in the circumſtances of theſe meaſures, and to mention the condition and powers of the teleſcopes that were uſed to obtain them.

Magnitude of the new Stars.

Obſervations on *Ceres* compared with a lucid diſk.

7-feet reflector.

April 1, 1802. Having placed a lucid diſk at a conſiderable diſtance from the eye, but ſo that I might view it with perfect diſtinctneſs, I threw the image of Mr. Piazzi's ſtar, ſeen in a 7-feet reflector, very near it, in order to have the projected picture of the ſtar and the lucid diſk ſide by ſide, that I might aſcertain their comparative magnitudes. I ſoon perceived that the length of my garden would not allow me to remove the diſk-micrometer, which muſt be placed at right

* By comparing its apparent diſk with that of the Georgian planet, it was eſtimated, that the real diameter of this new ſtar could not amount to three-eighths of that of our moon.

angles

angles to the telescope, far enough to make it appear no larger than the star; and, not having disks of a less diameter prepared, I placed the smallest I had as far from me as the situation of the star would allow. Then, bringing its image again by the side of the disk, and viewing, at the same time, with one eye the magnified star, while the other eye saw the lucid disk, I perceived that Ceres, which is the name the discoverer has given to the star, was hardly more than one third of the diameter of the disk, and certainly less than one half of it.

This being repeated, and always appearing the same, we shall not under-rate the size of the star, by admitting its diameter to have been 45 hundredths of the lucid disk.

The power of the telescope, very precisely ascertained by terrestrial geometrical measures properly reduced to the focus of the mirror on the stars, was 370,42. The distance of the lucid disk from the eye, was 2131 inches; and its diameter 3,4 inches. Hence we compute, that the disk was seen under an angle of 5′ 29″,09; and Ceres, when magnified 370 times, appearing, as we have shewn, 45 hundredths of that magnitude, its real diameter could not exceed 0″,40. Had this diameter amounted to as much as was formerly estimated, the power of 370 would have made it appear of 6′ 10″, which is more than the whole lucid disk.

Power 370,42.

Diameter of Ceres 0″,40.

This extraordinary result raised in me a suspicion, that the power 370 of a 7-feet telescope, and its aperture of 6,3 inches, might not be sufficient to shew the planet's feeble light properly. I therefore adapted my 10-feet instrument to observations with lucid disks; which require a different arrangement of the head of the telescope and finder: I also made some small transparencies, to represent the object I intended to measure.

April 21. The night being pretty clear, though perhaps not quite so proper for delicate vision as I could have wished, I directed my 10-feet reflector, with a magnifying power of 516,54, also ascertained by geometrical terrestrial measures reduced to the focus of the instrument on celestial objects, to Mr. Piazzi's star, and compared it with a lucid disk, placed at 1486 inches from the eye, and of 1,4 inch in diameter. I varied the distance of the lucid disk many times; and fixed at last on the above-mentioned one, as the best I could find. There was, however,

2. Observation repeated on Ceres with 10-feet reflector. Power 516,54.

Hazineſs.

however, a hazineſs about the ſtar, which reſembled a faint coma; and this, it may be ſuppoſed, muſt render the meaſure leſs ſatisfactory than it would otherwiſe have been.

Diameter of Ceres $0'',38$.

From theſe data we compute, that the diſk appeared to the natural eye under an angle of $3'\ 14'',33$; while Ceres, when magnified $516\frac{1}{2}$ times, was ſeen by the other eye of an equal magnitude; and that conſequently its real diameter, by meaſurement, was only $0'',38$.

3. Obſervation repeated on Ceres; 10-feet reflector, new ſmall mirror;

April 22. $11^h\ 38'$, ſidereal time. I uſed now a more perfect ſmall mirror; the former one having been injured by long continued ſolar obſervations. This gave me the apparent diameters of the ſtars uncommonly well defined; to which, perhaps, the very favourable and undiſturbed clearneſs of the atmoſphere might contribute conſiderably.

power 881,51;

With a magnifying power of 881,51, properly aſcertained, like thoſe which have been mentioned before, I viewed Dr. Olbers's ſtar, and compared it with a lucid diſk of 1,4 inch in diameter, placed at 1514 inches from the eye, meaſured, like the reſt of the diſtances, with long deal rods. The ſtar appeared to me ſo ill defined, that, aſcribing it to the eye-glaſs, I thought it not adviſeable to compare the object, as it then appeared, with a well defined lucid diſk. Exchanging the glaſs for that which gives the teleſcope a magnifying power of $516\frac{1}{2}$, I found Pallas, as the diſcoverer wiſhes to have it called, better defined; and ſaw, when brought together, that it was conſiderably leſs in diameter than the lucid diſk.

bad obſ.

Power changed to $516\frac{1}{2}$.

Pallas,

In order to produce an equality, I removed the diſk to 1942 inches; and ſtill found Pallas conſiderably leſs than the diſk.

ſmaller than Ceres.

Before I changed the diſtance again, I wiſhed to aſcertain whether Ceres or Pallas would appear under the largeſt angle, eſpecially as the air was now more pure than laſt night. On comparing the diameter of Ceres with that of the lucid diſk, I found it certainly leſs than the diſk. By proper attention, and continued examination, for at leaſt an hour, I judged it to be nearly $\frac{3}{4}$ of the lucid diſk.

Then, if we calculate as before, it appears by this obſervation, in which there is great reaſon to place confidence, that the angle under which this ſtar appeared, was only $0''.22$. For, a lucid diſk of 1,4 inch diameter, at the diſtance of 1942

inches,

inches, would be ſeen under an angle of 2′ 28″,7; three quarters of which are 1′ 51″,52. This quantity, divided by the power 516,54, gives 0″,2159, or, as we have given it abridged, 0″,22.

Excellent reſult of Ceres, diam. 0″,22.

13ʰ 7′. I removed the micrometer to the greateſt convenient diſtance, namely, 2136 inches, and compared Dr. Olbers's ſtar, which, on account of its great altitude, I ſaw now in high perfection, with the lucid diſk. It was, even at this diſtance, leſs than the diameter of the diſk, in the proportion of 2 to 3.

When, by long continued attention, the appearance of Pallas was reduced to its ſmalleſt ſize, I judged it to bear no greater proportion to the diameter of the lucid diſk of the micrometer, than as 1 to 2.

In conſequence of theſe meaſures, it appears that the diameter of Pallas, according to the firſt of them, is 0″,17; and, according to the laſt, where the greateſt poſſible diſtinctneſs was obtained, only 0″,13.

Diam. of Pallas 0″,13.

If it ſhould appear almoſt incredible that theſe curious objects could give ſo ſmall an image, had they been ſo much magnified as has been reported, I can ſay, that curioſity led me to throw the picture of Jupiter, given by the ſame teleſcope and magnifying power, on a wall at the diſtance of 1318 inches, of which it covered a ſpace that meaſured 12 feet 11 inches. I do not mention this as a meaſure of Jupiter, for the wall was not perfectly at right angles to the teleſcope, on which account the projected image would be a little larger than it ſhould have been, nor was I very attentive to other neceſſary minute circumſtances, which would be required for an accurate meaſure; but we ſee at once, from the ſize of this picture, that the power of the teleſcope exerted itſelf to the full of what has been ſtated.

Proof by the image of Jupiter that the power of the teleſcope was truly as here ſtated.

As we generally can judge beſt of comparative magnitudes, when the meaſures are, as it were, brought home to us; it will not be amiſs to reduce them to miles. This, however, cannot be done with great preciſion, till we are more perfectly acquainted with the elements of the orbits of theſe ſtars. But, for our preſent purpoſe, it will be ſufficiently accurate, if we admit their mean diſtances from the ſun, as the moſt recent information at preſent ſtates them; for Ceres 2,6024; and for Pallas 2,8. The geocentric longitudes and north latitudes, at the time of

Rough computation of the diameters of theſe bodies in miles:

of obſervation, were, for Ceres, about ♍ 20° 4′, 15° 20′; and for Pallas, ♍ 23° 40′, 17° 30′, With theſe data, I have calculated the diſtances of the ſtars from the earth at the time of obſervation, partly by the uſual method, and, where the elements were wanting, by a graphical proceſs, which is ſufficiently accurate for our purpoſe. My computed diſtauces were 1,634 for Ceres, and 1,8333 for Pallas; and, by them we find, that the diameter of Ceres, at the mean diſtance of the earth from the ſun, would ſubtend an angle of 0″,35127; and that, conſequently, its real diameter is 161,6 miles.

That of Ceres proves to be 161,6 miles; and that of Pallas 147 or 110½ miles.

It alſo follows, that Pallas would be ſeen, at the ſame diſtance from the ſun, under an angle of 0″,3199; and that its real diameter, if the largeſt meaſure be taken, is 147 miles; but, if we take the moſt diſtinct obſervation, which gives the ſmalleſt meaſure, the angle under which it would be ſeen from the ſun, will be only 0″,2399; and its diameter, no more than 110⅓ miles.

Of Satellites.

Examination for diſcovering Satellites.

None were aſcertained.

After what has juſt now been ſhewn, with regard to the ſize of theſe new ſtars, there can be no great reaſon to expect that they ſhould have any ſatellites. The little quantity of matter they contain, would hardly be adequate to the retention of a ſecondary body; but, as I have made many obſervations with a view to aſcertain this point, it will not be amiſs to relate them.

Feb. 25. 20-feet reflector. There is no ſmall ſtar near Ceres, that could be ſuppoſed to be a ſatellite.

Feb. 28. There is no ſmall ſtar within 3 or 4 minutes of Ceres, that might be taken for a ſatellite.

March 4. 9^h. 45′. ſidereal time. A very ſmall ſtar, ſouth-preceding Ceres, may be a ſatellite. See Plate V. Fig. 1. where C is Ceres, S the ſuppoſed ſatellite, *a b c d e f*, are delineation ſtars, *c* and *d* are very ſmall. S makes nearly a right angle with them; *e* is larger than either *c* or *d*. There is an extremely faint ſtar *f*, between *e* and *d*.

14^h. 16′. Ceres has left the ſuppoſed ſatellite behind.

March 5. There are two very ſmall ſtars, which may be ſatellites; ſee Fig. 2. where they are marked, 1ſt S, 2d S. The reſt, as before, are delineation ſtars.

March 6. The two ſuppoſed ſatellites of laſt night remain in their ſituation, Ceres having left them far behind.

10^h

10^h. 16′. There is a very ſmall ſtar, like a ſatellite, about 75° ſouth-following Ceres. See Fig. 3. It is in a line from C to *b* of laſt night.

11$_h$. 20′. Ceres has advanced in its orbit; but has left the ſuppoſed ſatellite behind.

March 30. 9^h. 35′. A ſuppoſed 1ſt ſatellite is directly following Ceres: it is extremely faint. A 2d ſuppoſed ſatellite is north-following. See Fig. 4. The ſuppoſed ſatellites are ſo ſmall, that, with a 20-feet teleſcope, they require a power of 300 to be ſeen; and the planet ſhould be hidden behind a thick wire, placed a little out of the middle of the field of view, which muſt be left open to look for the ſuppoſed ſatellites.

12^h 17′. Ceres has changed its place, and left both the ſuppoſed ſatellites behind.

March 31. 9^h 20′. There is a very ſmall ſtar, on the north-preceding ſide of Ceres, which may be a ſatellite.

11^h 50′. Ceres has moved forwards in its path; but the ſuppoſed ſatellite remains in its former ſituation. The neareſt ſtar is 20″ of time from Ceres; ſo that, within a circle of 40″ of time, there certainly is no ſatellite that can be ſeen with the ſpace-penetrating power of this inſtrument.

It is evident, that when the motion of a celeſtial body is ſo conſiderable, we need never be long in doubt whether a ſmall ſtar be a ſatellite belonging to it, ſince a few hours muſt decide it.

May 1. 12^h 51′. I viewed Pallas with the 20-feet reflector, power 300; there was no ſtar within 5′, that could be taken for a ſatellite.

Of the Colour of the new Stars.

Feb. 13. The colour of Ceres is ruddy, but not very deep.
April 21. Ceres is much more ruddy than Pallas.
April 22. Pallas is of a duſky whitiſh colour.

Ceres is of a ruddy colour; Pallas duſky white.

Of the Appearances of the new Stars, with regard to a Diſk.

Feb. 7. Ceres, with a magnifying power of 516½, ſhews an ill defined planetary diſk, hardly to be diſtinguiſhed from the ſurrounding hazineſs.

The diſk of Ceres is ill-defined:

Feb. 13. Ceres has a viſible diſk.

April

April 22. In viewing Pallas, I cannot, with the utmoſt attention, and under the moſt favourable preſent circumſtances, perceive any ſharp termination which might denote a diſk; it is rather what I would call a nucleus.

Pallas is not a diſk but a nucleus.

April 28. In the finder, Pallas is leſs than Ceres. It is alſo rather leſs than when I firſt ſaw it.

Of the Appearances of the new Stars, with regard to an Atmoſphere, or Coma.

Ceres has moſt frequently a coma (perhaps effected by the inſtrument, or more probably by the atmoſphere.)

April 21. I viewed Ceres for nearly an hour together. There was a hazineſs about it, reſembling a faint coma, which was, however, eaſily to be diſtinguiſhed from the body.

April 22. I ſee the diſk of Ceres better defined, and ſmaller than I did laſt night. There does not ſeem to be any coma; and I am inclined to aſcribe the appearance of laſt night to a deception, as I now and then, with long attention, ſaw it without; at which times it was always beſt defined, and ſmalleſt.

April 28. Ceres is ſurrounded with a ſtrong hazineſs. Power 550.

With $516\frac{1}{2}$, which is a better glaſs, the breadth of the coma beyond the diſk may amount to the extent of a diameter of the diſk, which is not very ſharply defined. Were the whole coma and ſtar taken together, they would be at leaſt three times as large as my meaſure of the ſtar. The coma is very denſe near the nucleus; but loſes itſelf pretty abruptly on the outſide, though a gradual diminution is ſtill very perceptible.

April 30. Ceres has a viſible, but very ſmall coma about it. This cannot be ſeen with low powers; as the whole of it together is not large enough, unleſs much magnified, to make up a viſible quantity.

May 1. The diameter of the coma of Ceres, is about 5 times as large as the diſk, or extends nearly 2 diameters beyond it.

$13^h\ 19'$. 20-feet reflector; power 477. The diſk of Ceres is much better defined than that of Pallas. The coma about it is conſiderable, but not quite ſo extended as that of Pallas.

May 2. $13^h\ 20'$. Ceres is better defined than I have generally ſeen it. Its diſk is ſtrongly marked; and, when I ſee it beſt, the hazineſs about it hardly exceeds that of the ſtars of an equal ſize.

Memorandum.

Memorandum. This may be owing to a particular dispoſition of the atmoſphere, which ſhews all the ſtars without twinkling, but not quite ſo bright as they appear at other times. Jupiter likewiſe has an extremely faint ſcattered light about it, which extends to nearly 4 or 5 degrees in diameter.

April 22. Pallas, with a power of 881½, appears to be very ill defined. The glaſs is not in fault; for, in the day time, I can read with it the ſmalleſt letters on a meſſage card, fixed up at a great diſtance. **Pallas is ill-defined, nebulous or cometary,**

13h 17′. The appearance of Pallas is cometary; the diſk, if it has any, being ill defined. When I ſee it to the beſt advantage, it appears like a much compreſſed, extremely ſmall, but ill defined, planetary nebula.

April 28. Pallas is very ill defined: no determined diſk can be ſeen. The coma about it, or rather the coma itſelf, for no ſtar appears within it, would certainly meaſure, at firſt ſight, 4 or 5 times as much as it will do after it has been properly kept in view, in order to diſtinguiſh between the hazineſs which ſurrounds it, and that part which may be called the body.

May 1. Pallas has a very ill defined appearance; but the whole coma is compreſſed into a very ſmall compaſs.

13h 5′. 20-feet reflector; power 477. I ſee Pallas well, and perceive a very ſmall diſk, with a coma of ſome extent about it, the whole diameter of which may amount to 6 or 7 times that of the diſk alone. **very ſmall diſk.**

May 2. 13h 0′. 10-feet reflector. A ſtar of exactly the ſame ſize, in the finder, with Pallas, viewed with 516½, has a different appearance. In the centre of it is a round lucid point, which is not viſible in Pallas. The evening is uncommonly calm and beautiful. I ſee Pallas better defined than I have ſeen it before. The coma is contracted into a very narrow compaſs; ſo that perhaps it is little more than the common aberration of light of every ſmall ſtar. See the memorandum to the obſervations of Ceres, May 2.

On the Nature of the new Stars.

From the account which we have now before us, a very important queſtion will ariſe, which is, What are theſe new ſtars, are they planets, or are they comets? And, before we can **Nature of the new ſtars.**

can enter into a proper examination of the ſubject, it will be neceſſary to lay down ſome definition of the meaning we have hitherto affixed to the term planet. This cannot be difficult, ſince we have ſeven patterns to adjuſt our definition by. I ſhould, for inſtance, ſay of planets,

Criterions of planets.

1. Magnitude.

1. They are celeſtial bodies, of a certain very conſiderable ſize.

2. Orbits nearly circular.

2. They move in not very excentric ellipſes round the ſun.

3. Nearly in the pl. ecliptic.

3. The planes of their orbits do not deviate many degrees from the plane of the earth's orbit.

4. Motion direct.

4. Their motion is direct.

5. Satellites or rings.

5. They may have ſatellites, or rings.

6. Atmoſphere, not extenſive.

6. They have an atmoſphere of conſiderable extent, which however bears hardly any ſenſible proportion to their diameters.

7. Orbits conſiderably aſunder.

7. Their orbits are at certain conſiderable diſtances from each other.

Compariſon of the new ſtars in theſe reſpects.

Now, if we may judge of theſe new ſtars by our firſt criterion, which is their ſize, we certainly cannot claſs them in the liſt of planets: for, to conclude from the meaſures I have taken, Mercury, which is the ſmalleſt, if divided, would make up more than 135 thouſand ſuch bodies as that of Pallas, in bulk.

In the ſecond article, their motion, they agree perhaps ſufficiently well.

The third, which relates to the ſituation of their orbits, ſeems again to point out a conſiderable difference. The geocentric latitude of Pallas, at preſent, is not leſs than between 17 and 18 degrees; and that of Ceres between 15 and 16; whereas, that of the planets does not amount to one half of that quantity. If bodies of this kind were to be admitted into the order of planets, we ſhould be obliged to give up the zodiac; for, by extending it to them, ſhould a few more of theſe ſtars be diſcovered, ſtill farther and farther deviating from the path of the earth, which is not unlikely, we might ſoon be obliged to convert the whole firmament into zodiac; that is to ſay, we ſhould have none left.

In the fourth article, which points out the direction of the motion, theſe ſtars agree with the planets.

(To be continued in our next.)

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MARCH, 1803

ARTICLE I.

Comparative Experiments and Observations on Myrtle Wax, Bees Wax, Spermaceti, Adipocire, and the Crystalline Matter of biliary Calculi. In a Letter from JOHN BOSTOCK, *M. D.*

To Mr. NICHOLSON.

Liverpool, Jan. 23, 1803.

SIR,

IN the course of last summer, I was requested by a friend, who had received from America a quantity of the peculiar vegetable matter called Myrtle Wax, to give him some information respecting its nature and properties, and the probable use to which it might be advantageously applied. For this purpose I examined several works on natural history and chemistry, but I could only meet with a general account of the method employed for obtaining it, with a vague description of its physical properties. Respecting its chemical analysis I could find nothing satisfactory; even the elaborate work of Fourcroy contains little precise information on this head *. I determined

Enquiry concerning the nature and uses of myrtle wax.

Scarcely mentioned by Fourcroy.

* Fourcroy, Systême des Connoissances Chimiques, tom. vii. p. 339.

determined therefore to examine this point more accurately, and with this intention, the experiments of which I send you an abstract were performed. Some other subjects however having at that time occupied me, I had almost forgotten my analysis of the myrtle wax, until in looking over the 131st No. of the *Annales de Chimie*, I found that the subject had been treated in an ample manner by M. Cadet. This circumstance induced me to recur to my Papers, in order to compare my experiments with those of M. Cadet; when I found that though his memoir contains a very complete account of the natural history of the substance, yet that my chemical analysis was more minute. On this account I conceive, that if you have any intention of inserting in your Journal * a translation of M. Cadet's essay, my communication may prove a useful supplement.

But more fully by Cadet.

I am,

Your obedient servant,

JOHN BOSTOCK.

Description and Analysis of Myrtle Wax.

Obvious properties of myrtle wax.

The peculiar substance obtained from the Myrica Cerisera of Linnæus †, and which has obtained the name of myrtle wax, is a concrete body, of moderate hardness and consistence; it has in part the tenacity of bees wax, though without its unctuosity; along with this, it also possesses, in some degree, the brittleness of the resins. The colour of the myrtle wax is a pale green; the shades of the different pieces are however somewhat varied; in most of them, the green has a tendency to a dirty grey; in others, it is lighter, more transparent, and of a yellowish tinge. Its specific gravity is about 1,0150, water being 1,0000, and white bees wax, 9600. It is fused at a temperature of 109°; by sufficiently increasing the heat, it burns with a peculiarly clear, white flame, produces little smoke, and during the combustion emits an agreeable aromatic odor.

Habitudes with reagents. Water has no action.

The following are its habitudes with the different re-agents.

1. Water has no action upon the myrtle wax, either when cold or at the boiling heat.

* The reader will find it in the present Number. N.

† Martyn's edition of Millar's Dictionary.

Alcohol,

2. Alcohol, at the ordinary temperature of the atmoſphere, has no action upon it; but 100 parts, by weight, of this fluid, when boiling, diſſolve about five parts of the wax. Nearly $\frac{4}{5}$ is depoſited by the cooling of the alcohol; $\frac{1}{5}$ ſtill remains ſuſpended; this is ſlowly depoſited in the courſe of a few days, or may be precipitated by the addition of water. This ſubſtance, when precipitated from the alcohol, is lighter coloured than in its original ſtate, and approaches more to a grey tinge. Though the myrtle wax ſeems to be homogeneous in its texture and conſiſtence, it is not totally ſoluble in alcohol: about $\frac{4}{5}$ only of the whole is acted upon by this fluid, even when boiling. The part which is inſoluble in alcohol, when ſeparated from the reſt of the maſs, exhibits a ſomewhat darker ſhade of green than before the experiment; the alcohol remains completely colourleſs.

Cold alcohol does not act upon it. Hot alcohol ſparingly diſſolves it in part only, and depoſits this by cooling and repoſe.

3. Sulphuric ether, when at the common temperature of the atmoſphere, diſſolves the myrtle wax only in ſmall quantity, but it acts upon it rapidly when boiling. On account of the tendency which the fluid has to evaporate, it is difficult to aſcertain the exact proportion, but it ſeems to take up ſomewhat more than $\frac{1}{4}$ of its own weight. The greateſt part of this is ſeparated as the ether cools, and the reſt may be precipitated by the addition of water. The wax, after it has been diſſolved in ether, is nearly colourleſs, while the fluid aſſumes a beautiful green hue. If the wax be not too abundant, and the ether be ſuffered to evaporate ſlowly, the wax is depoſited on the ſides of the glaſs in a cryſtalline or lamellated form; in this ſtate its texture approaches ſomewhat to that of ſpermaceti.

Cold S. Ether diſſolves little; hot ether more rapidly and largely. The depoſit by evaporation reſembles ſpermaceti.

4. Rectified oil of turpentine at the temperature of the atmoſphere ſoftens the wax, but does not ſeem capable of diſſolving it; when aſſiſted by a moderate heat, 100 grains of the turpentine diſſolve ſix grains of the myrtle wax. The turpentine acquires a light green tinge, part of the wax is ſeparated as the fluid cools, while part remains permanently diſſolved in it.

Ol. Turpentine acts but ſlightly.

5. When the myrtle wax is boiled with liquid cauſtic potaſh, the fluid becomes turbid, but after ſome time the greateſt part of the wax riſes to the ſurface, nearly without colour, in a flocculent form. A ſmall quantity of it only remains diſſolved

Pure potaſh in water renders it colourleſs by boiling, and forms a ſoap with a ſmall part, which be-

ing decomposed by acid affords the wax nearly unchanged.

ſolved in the potaſh; this may be precipitated from it by an acid. That part of the wax which riſes to the ſurface is converted into a ſaponaceous matter; it has loſt its inflammability and fuſibility, and forms an opake ſolution with water. From this ſolution it is precipitated by an acid in the form of white flakes, which when collected, will be found to reſemble very nearly the wax before its union with the potaſh.

Pure ammonia acts nearly as potaſh, but more feebly.

6. Pure ammonia exhibits with the myrtle wax phenomena in many reſpects, ſimilar to thoſe produced by the fixed alcalis. When its action is promoted by heat, an opake ſolution is produced; the wax is deprived of its colour; the greateſt part of it ſeparates from the fluid, and is converted into a ſubſtance partly ſoluble in warm water, though leſs ſo than that reſulting from the action of potaſh upon myrtle wax.

The three principal mineral acids act upon it; though not with any notable force.

7. The mineral acids exerciſe little action upon this ſubſtance at the ordinary temperature of the atmoſphere; the ſulphuric diſſolves it ſparingly, and acquires a brown tinge. With the aſſiſtance of a moderate heat this acid diſſolves about $\frac{1}{12}$ of its weight, and is converted into a thick, dark-brown maſs; by cooling it becomes nearly concrete, but no ſeparation of the wax takes place. The nitric and muriatic acids, even when heated, ſeem to poſſeſs little attraction for the myrtle wax. After the wax had been kept for ſome time in a ſtate of fuſion in contact with the nitric acid, its green hue was converted into a pale yellow, but the acid remained colourleſs, nor did any part of the wax appear to be diſſolved in it: the wax by long digeſtion in muriatic acid became of a bright orange colour.

Deduction: that myrtle wax appears to be an oxigenated fixed oil.

Theſe experiments will enable us, at leaſt with a conſiderable degree of probability, to aſſign the place which the vegeable myrtle wax muſt hold in a natural arrangement of chemical ſubſtances. Its inflammability, fuſibility, its inſolubility in water, and the action which takes place between it and the alcalis, point out its affinity to the fixed oils, while its texture and conſiſtence, and more particularly its habitudes with alcohol and ether, indicate a reſemblance to the reſins. We may therefore conſider the myrtle wax as a fixed vegetable oil, rendered concrete by the addition of a quantity of oxigen; it ſeems to hold the ſame relation to the fixed, that reſins do to the eſſential oils of vegetables.

But

But though the myrtle wax be itſelf of vegetable origin, there are ſome animal ſubſtances which more nearly reſemble it in its chemical properties than any product of the vegetable kingdom. The principal of theſe is the wax elaborated by the bee, to which the peculiar ſubſtance now under conſideration bears a ſtrong reſemblance, both in its phyſical and chemical properties. Myrtle wax alſo in many particulars reſembles ſpermaceti; the ſubſtance called Adipocire, produced by the action of nitric acid upon the muſcular fibre; and the cryſtalline matter of biliary calculi. I ſhall in a very conciſe manner, inſtitute a compariſon between myrtle wax and each of theſe ſubſtances.

But it reſembles the wax of the bee; and alſo ſpermaceti; and the adipocire produced by nitric acid upon muſcular fibre; and the cryſtalline matter of biliary calculi.

Bees Wax.

This ſubſtance, in its phyſical properties, differs from myrtle wax in being more unctuous, and poſſeſſing a greater degree of tenacity: its colour and ſmell are alſo different. Bees wax is likewiſe conſiderably leſs fuſible; Dr. Pearſon * and Mr. Nicholſon † fix its melting point at 142°; whereas Mr. Fourcroy ‡ places it lower in the ſcale at 117°; on this ſubject the reſults of my experiments coincide exactly with thoſe of the Engliſh chemiſts. There is alſo a difference of opinion among chemiſts reſpecting the action which takes place between this ſubſtance and alcohol; Fourcroy §, Chaptal ||, and Nicholſon **, aſſert that it is inſoluble in this fluid, while Pearſon maintains the contrary; on this queſtion my experience agrees with that of Dr. Pearſon. The proportion of bees wax which the alcohol is capable of diſſolving, ſeems however to be ſomewhat leſs than that of the myrtle wax. As in the former caſe, the greateſt part of the wax ſeparates as the fluid cools; while the remainder may be precipitated by the addition of water. Bees wax is ſparingly diſſolved by boiling ether, leſs readily, and in conſiderably leſs proportion

Compariſon with bees wax. Myrtle wax is more unctuous; tenacious; different in colour and ſmell; more fuſible, more ſoluble in alcohol and in ether; more affected by potaſh, and nearly the ſame by ammonia.

* Pearſon's Obſervations and Experiments on White Lac. Phil. Tranſ. 1794.

† Nicholſon's Journal, Quarto, vol. i. p. 70.

‡ Fourcroy Syſtême des Connaiſſ. Chimiques, x. 343.

§ Ibid. and Thomſon's Fourcroy, Vol. iii. p. 387.

|| Chaptal's Elements, Vol. III. p. 164.

** (Nicholſon's Elements, p. 502.)

than

than the myrtle wax; this fluid, when heated, feems only to take up about $\frac{1}{20}$ of its weight of bees wax. Cauftic potafh exhibits the fame phenomena with bees wax as with the product of the myrica cerifera; it was converted into the faponaceous ftate, and became foluble in warm water. It appeared however that the action was lefs violent, and the change lefs complete than in the former cafe. Ammoniac, when boiling, readily forms with bees wax an emulfion, in fome refpects refembling that produced by the fame fubftance with the myrtle wax. As the mixture cools, the greateft part of the wax rifes to the furface in a flocculent form; it appears to have fo far contracted a union with the alcali as to have its texture and odour deftroyed, and its fufibility and inflammability diminifhed; yet it is little, if at all, foluble in water.

Spermaceti.

Spermaceti: Is more fufible than either of the preceding.

One property of this fubftance, which obvioufly diftinguifhes it from thofe already defcribed, is the cryftalline texture which it conftantly affumes. It is more fufible than either of the fubftances which we have examined; but refpecting the precife temperature at which it becomes liquid, there is a confiderable difference of opinion. Fourcroy * ftates it to be at the 98th degree, or a little lower †; Mr. Nicholfon ‡ fuppofes it to be at the 133d degree; I have found the melting point of fpermaceti to be uniformly 112°; there may perhaps be a real difference in the fpecimens that have been employed.

Sparingly foluble in alcohol,

Like the two kinds of wax, it is foluble in alcohol, though very fparingly; according to my experiments, it required a quantity of boiling alcohol equal to 150 times the weight of the fpermaceti to diffolve it; a proportion which nearly coincides with the eftimate of Fourcroy: the whole is precipitated as the fluid cools.

rapidly by hot ether,

It is rapidly diffolved by warm ether, by cooling it is precipitated fo plentifully, as in appearance to convert the whole into a folid cryftallized mafs.

and oil of turpentine.

Spermaceti is alfo diffolved with great facility by oil of turpentine gently heated, but is depofited from it as it cools.

More ftrongly attacked by potafh than m. or b. wax.

It unites very readily with cauftic potafh, and the compound is completely

* Encyc. Math. Chimie, Art. Blanc de Baleine.

† Annales de Chim. tom. vii. 192.

‡ Nicholfon's Journal, Vol. I. 4to, p. 70.

foluble

ſoluble in warm water: potaſh ſeems to exerciſe upon ſpermaceti a more powerful action, than upon either the myrtle or the bees wax. Ammoniac, at the uſual temperature of the atmoſphere, does not appear to exerciſe any action upon ſpermaceti, but when boiling it unites with it readily, and forms an emulſion, which is not decompoſed by the cooling of the mixture, or by the addition of water; but the ſpermaceti is inſtantly precipitated by the addition of an acid. No important phenomena reſult from the action of the mineral acids upon ſpermaceti.

Strongly by hot ammonia, and then not decompoſ. by cooling or dilution, but by an acid.

Adipocire.

Adipocire. Deſcription.

I procured a quantity of this ſubſtance by digeſting diluted nitric acid upon the muſcular fibre; it was afterwards waſhed in warm water, in order to ſeparate any portion of adhering acid. The matter thus purified, was of a light yellow colour, of about the conſiſtence of tallow, and of a homogeneous texture. Reſpecting the temperature at which it is fuſed, we meet with the ſame uncertainty as in the former caſes. Fourcroy in one of his eſſays * fixes its melting point at the 98th degree; the ſame author in another place ſtates it to be the 110th †, while Mr. Nicholſon ‡ ſuppoſes it to be as high as the 127th; in Dr. Rees's Cyclop. § it is ſtated, that this ſubſtance melts at 7° below ſpermaceti, which according to my eſtimate would be the 105th degree. In my own experience upon this ſubject it became liquid at the 92d degree. Alcohol, at the ordinary temperature of the atmoſphere, diſſolves it only in ſmall quantity, but by the aſſiſtance of a gentle heat it acts upon it with rapidity. Fourcroy || ſtates that this fluid when boiling diſſolves about its own weight of adipocire, $\frac{1}{4}$ or $\frac{1}{5}$ of which is retained after the fluid cools. The ſame chemiſt in another memoir aſſerts, that one ounce of alcohol will diſſolve 12 drams of this ſubſtance **. There may probably be ſome difference in the chemical nature of adipocire, according

Fuſibility.

Solubility in hot alcohol, conſiderable.

* Ann, de Chimie, tom. vii. 192.

† Ann de Chimie, tom. viii. 66.

‡ Nicholſon's Journal, ubi ſupra.

§ Rees's Cyclop. new edit. Art. Adipecire, this is probably inſerted only upon the authority of Fourcroy.

|| Ann. de Chimie, VII. 191.

** Ann. de Chimie, VIII. 67.

to

to the procefs by which it is obtained, or the rapidity of its production; in my experiments the quantity which the alcohol was capable of diffolving, though very confiderable, was certainly lefs than that ftated by Fourcroy. The greater part is depofited as the fluid cools, and the remainder may be precipitated by water. The adipocire, after this operation, is rendered nearly white, while the alcohol affumes a deep yellow tinge. Ether diffolves it fparingly, when unaffifted by heat; when boiling it takes up about ¼ of its own weight; this is for the moft part feparated by the cooling of the fluid; the adipocire is depofited nearly white, while the ether acquires a yellowifh green colour. The cauftic alcalis, both fixed and volatile, exert upon this fubftance the fame kind of action which we have defcribed in the former inftances; when heated in contact with it, they form a faponaceous emulfion of a reddifh brown colour, which is mifcible with water without decompofition. The volatile alcali diffolves it fparingly, without the affiftance of heat; a circumftance in which the adipocire differs both from any of the fubftances which we have hitherto examined, and alfo from the cryftalline matter of biliary calculi. Upon the whole the adipocire is more fufible, more inflammable, and more eafily acted upon by the different reagents than any fubftance which has paffed under our review.

and in ether.

Alkalis as before.

Ammonia diffolves it cold.

Cryftalline Matter of Biliary Calculi.

Cryftals of biliary calculi. Defcription.

It now only remained to perform fome comparative experiments with the cryftalline matter of biliary calculi, and I was fortunately in poffeffion of two of thefe bodies, which were prefented to me by Dr. Gerard of this place. The calculi were fimilar in their texture and appearance, and of nearly the fame fize; the one which I examined was of an irregular, polyhedral figure, with its edges and angles blunted; it was fomewhat brittle, and of an ochry brown colour. Its fpecific gravity was about ,9000; it weighed 16 grains. When broken, and viewed through a microfcope, it was found to confift of an internal nucleus, formed of radii converging to a centre, and of an external cruft compofed of four or five thin ftrata. It evidently confifted of two diftinct fubftances; one white, fparkling, and of a cryftalline texture, by which its general ftructure was determined; the other a number of dark coloured particles, irregularly difperfed through the interftices

of

of the former. I therefore concluded it to be that kind of calculus which Fourcroy calls Cyſtic-Adipobileous *; compoſed of the peculiar cryſtallized matter, and of particles of inſpiſſated bile, mixed together in different proportions. The cryſtalline matter is fuſible and inflammable, but the preciſe degree at which it melts has not been aſcertained; it is not even ſoftened by the heat of boiling water. Alcohol in the cold has no action upon it, but when boiling it diſſolves it with facility; Fourcroy † ſtates, that one part of this ſubſtance is taken up by 19 parts of the fluid; this however was not the caſe with the calculus which I examined; in this inſtance the alcohol certainly did not diſſolve more than $\frac{1}{30}$ of its weight of the cryſtalline matter. As the fluid cools, the ſubſtance is depoſited in the form of white, ſhining ſpiculæ, intermixed with thin plates. Ether diſſolves it ſlowly in the cold, but more readily when heated; the greateſt part is depoſited as the fluid cools, and the reſt may be precipitated by water. If the ether be ſuffered to evaporate ſlowly at the ordinary temperature of the atmoſphere, the matter which it held in ſolution will be depoſited on the ſides of the glaſs, in the form of beautiful radiated cryſtals. Oil of turpentine acts upon this matter with difficulty; it appears, however, when digeſted with it for ſome time at the boiling heat, to diſſolve it in a ſmall degree. It is acted upon by cauſtic potaſh when boiling, and the reſult of their union appears to be of the ſame nature with that deſcribed in the former inſtances. A ſmall quantity is diſſolved by the pot-aſh, and may be precipitated by an acid; while another part is converted into a ſubſtance ſoluble in water, but inſoluble in alcohol; it may be precipitated from the water by an acid. Ammoniac, even when boiling, ſeems to poſſeſs little or no attraction for this cryſtalline matter.

Fuſible and inflammable; not acted on by water.

Hot alcohol diſſolves it ſparingly,

ether more fully.

Ol. turpentine little.

Fixed alkalis as before, and ammonia ſcarcely at all.

The reſults of ſeveral experiments oblige me to differ from Dr. Powel, reſpecting the effect of the fixed alcalis upon this peculiar ſubſtance; he conceives that it is not acted upon by them ‡; the opinion which I have adopted is however ſupported by the authority of Fourcroy §. Nitric acid, more particularly

Nitric acid acts upon and alters it.

* Fourcroy, Syſtême des Conn. Chim. x. 59.

† Ann. de Chimie, VII.

‡ Powel on the Bile, 119.

§ Encyc. Meth. Chimie; art. Bile; Fourcroy, Syſtême des Conn. Chimiques, x.

particularly when affifted by heat, acts readily upon the cryftalline matter; during the procefs there is a difengagement of nitrous gas. A fmall quantity remains diffolved in the fluid, and may be precipitated from it by potafh. The greateft part however rifes to the furface as the fluid cools in the form of drops of oil, which gradually grow concrete; the cryftalline texture is deftroyed, and its confiftence refembles that of a vegetable refin. Water does not diffolve this peculiar matter, but it feems to render it fomewhat more brittle and friable. Alcohol affifted by a gentle heat diffolved it; it was precipitable from the folution by water in the form of a grey powder. Ether, at the temperature of the atmofphere, diffolved it rapidly; water precipitated it from the ether in the form of drops of oil. When the fluid was evaporated, it was depofited, without exhibiting any marks of a cryftalline ftructure. Cauftic potafh acted upon it without the affiftance of heat, when boiling it diffolved it with more facility; the fluid acquired a reddifh brown hue; it was not precipitated by water, but the fulphuric acid feparated it in the form of a grey powder. The action of ammoniac was nearly fimilar, though as I conceived, fomewhat more powerful than that of the fixed alcalis. The folution was alfo of a reddifh brown colour; but the precipitate by fulphuric acid was of a bright yellow. It appears therefore that the matter of biliary calculi has its properties materially changed by the operation of the nitric acid. It entirely deftroys its cryftalline tendency, and renders it more foluble in ether and in the alcalis. It has been fuppofed that by this procefs it becomes more affimilated to the adiporefin of the bile *, but it ftill differs from it in not poffeffing any degree of folubility in water †.

Habitudes of the altered fubftances.

These remarks upon the cryftalline matter of biliary calculi, fhew that there are feveral important circumftances in which it differs both from fpermaceti, and from adipocire, to both of which it has been compared ‡. Upon the whole, though the

The fubftances here treated are really different.

* Powel, p. 121.

† Ann. de Chimie, tom. vii. p. 178. Encyc. Meth. Chimie, art. Bile, p. 566.

‡ I may obferve that the dark-coloured particles which were difperfed through the cryftals of the calculus, though they might have been originally compofed of infpiffated bile, did not retain the properties of this fubftance, as they were nearly infoluble both in boiling water and in alcohol.

five

five subſtances which have paſſed under our review poſſeſs certain properties in common, and have a degree of ſimilarity in their external appearance, yet they differ materially in their chemical nature. There is indeed reaſon to conjecture that they are all compoſed of the ſame elements, combined together in different proportions and with different degrees of attraction.

Lavoiſier * firſt made us acquainted with the chemical compoſition of oil, and proved that it conſiſts of hydrogene and carbone. This great philoſopher alſo demonſtrated that wax differs from oil, in containing a greater proportion of carbone; there is every reaſon to ſuppoſe, that a quantity of oxygene likewiſe enters into its compoſition. It may be conjectured, that the five ſubſtances which have paſſed under our review, differ from each other in the proportion of oxygene, hydrogene, and carbone; but the preſent ſtate of our knowledge will not enable us to determine how far the individual properties will be affected by the different proportions. It had been conjectured, that an addition of carbone renders a body leſs fuſible, and at the ſame time more ſoluble in alcohol; but we find from the experiments recited above, that adipo-cire, which is the moſt fuſible, is likewiſe the moſt ſoluble in alcohol. Probably a good deal may depend in theſe caſes upon the ſtate of the combination of the ingredients, as well as upon their proportions. General remarks.

II.

On the Nature of Muſical Sounds. In Reply to Dr. Young. *By Mr.* John Gough.

To Mr. NICHOLSON.

SIR,

THE controverſy between Dr. Young and me has taken a new turn in your laſt number; for my opponent, with the addreſs of an able politician, endeavours to ſecure the victory by the aid of a powerful auxiliary. For this purpoſe, he has attempted to oppreſs my cauſe, and ſilence my reaſons, by the authority of an illuſtrious name; whom he has impreſſed into his Introductory remark.

* Lavoiſier, Mem. Acad. Scienc. 1784.

his ſervice. The only plea that I can offer in oppoſition to this formidable confederacy, conſiſts in the following plain remark: I contend for the truth and not for ſuperiority, a motive which obliges every diſputant to diſregard all authority, except the conviction impreſſed on the ſenſes by a judicious experiment, and the aſſent given by the underſtanding to ſound arguments. Notwithſtanding the preceding declaration, I acknowledge the weight of perſonal reſpectability, in many inſtances of an experimental nature; becauſe the reputation of one philoſopher ſtands higher than that of another does, as a conductor of experiments; on this ſuppoſition, the deciſions of the former ought to be preferred to thoſe of the latter, when they happen to diſagree in their concluſions; in fact every man's confidence in the juſtice of an experiment which he has not ſeen, amounts to nothing more than hiſtorical evidence, the credit of the narrative reſting entirely upon the character and abilities of the eye witneſs.

Perſonal reſpectability is of weight in hiſtory;

The foregoing obſervations on experimental knowledge have nothing to do with argument; for every ſpecies of true logic, including the mathematics, conſiſts of a train of inductions; all of which are drawn from maxims, admitted by every party, in a well conducted diſpute. On this account, argumentation is not an appeal to a perſon's faith, but to his rational faculties; and an unprejudiced judge will prefer the logic of a ploughman to that of Ariſtotle himſelf, ſhould the ruſtic happen to reaſon better than this maſter of the art. After what has been ſaid on the weight of authority in philoſophical controverſy, it will not be expected that I ſhall pay the leaſt deference to the dogma of any man in the world, unleſs his opinion is ſupported by demonſtration; for this is the only literary authority to which an enquirer after the truth ought to bow. Conſiſtently with this declaration, an attempt has been made to demonſtrate every propoſition that has been advanced on my part, in the preſent diſpute; a diſpute which commenced in the tranſactions of the Royal Society, which paſſed from that publication to the Mancheſter Memoirs, and is now prolonged, Sir, in your Journal. But my care in this reſpect, and attachment to proof have not been imitated by my opponent; for nothing appears in the courſe of the controverſy, reſembling a refutation of any one of my concluſions, excepting an objection which was offered in Dr. Young's laſt letter, to

but not in argument or reaſoning.

The preſent controverſy not determinable by authority.

to the theory of the grave harmonics. The Doctor obſerves, that two uniſons, the vibrations of which biſect each other, ought to give the octave, according to my principles. To this I take the liberty to reply, that no two perfect uniſons produce a cycle; becauſe the arrangement which is given to their firſt vibrations, will evidently prevail to the end of the experiment: no points of diviſion can therefore be formed for the ſeparation of the contiguous cycles; in other words, no cycles will be produced, conſequently no imaginary ſound can be perceived in the courſe of the Doctor's experiment, becauſe the exiſtence of the grave harmonics depends upon a ſucceſſion of minute cycles, if my idea of their origin be correct. The effect of two imperfect uniſons will perhaps appear upon examination, not leſs unfavourable to the Doctor's objection, than the preceding caſe: conſonances of this deſcription, it is true, always run into cycles; but then they are too long to give riſe to continuous ſounds, being better fitted to produce beats. For inſtance, let two homogeneous cords of equal diameters be ſtretched with equal forces; and ſuppoſe their lengths to be 20 and 20,1 inches, then the longer will make 200 vibrations, while the ſhorter compleats 201. But the cord correſponding to the grave harmonic of this conſonance, vibrates but once in the ſame time; therefore the length of it is to 20,1 inches as 200 to 1; or it is 335 feet. The difficulty of conducting ſuch an experiment muſt be obvious to every one; for whether ſtrings or pipes be uſed, the lower note of this conſonance muſt compleat at leaſt twelve cycles, or 2400 vibrations in a ſecond; otherwiſe a grave harmonic cannot reſult from this combination of imperfect uniſons.

That two uniſons cannot give the octave by biſection, inſiſted becauſe they cannot form a cycle.

Imperfect uniſons produce beats.

Having now replied to the Doctor's objection, apparently in a ſatisfactory manner, I may without the imputation of rudeneſs, requeſt him to conſider in what way the ſeveral properties of theſe ſecondary ſounds are to be reconciled to his ſyſtem, more eſpecially their want of a fixed direction: ſhould he fail in the attempt, he needs not be reminded that every theory is imperfect, which explains a part of a phenomenon, but does not embrace the whole of it. The want of direction appeared to me ſo plain a proof of the nature of the grave harmonics, the firſt time I made the experiment, that I immediately explained the fact on the principles, which are given at the beginning of this volume of your Journal, without once ſuppoſing the theory

How the want of direction, &c. can be reconciled to Dr. Young's ſyſtem?

to be new; before Dr. Young's laſt letter informed me of the circumſtance. Every attempt to account for this ſingular effect of conſonances by pulſes tranſmitted to the ear through the air, will in all probability encounter the obſtacle already mentioned: for the notion of force compriſes the idea of direction, as well as that of power; and if the latter property cannot be uſed independent of the former, every theory founded on this principle muſt be contradicted by obſervation. I do not pretend to form a judgment on the work of M. Lagrange, from Dr. Young's extracts; and perſons reſiding at a diſtance from the metropolis, as I do, ſeldom enjoy acceſs to uncommon books, ſuch as the Miſcellanea Taurinenſia. However, I agree to the propoſition of M. Lagrange, that a particle of air agitated by two ſounds, acquires a motion differing from that, which each of them would give to it ſeparately: at the ſame time I muſt withhold my aſſent to the ſequel, if I underſtand it rightly; viz. that the ſounds coaleſce in conſequence of this motion, until my arguments on this ſubject have been refuted. While this remains unattempted, the prolongation of the preſent controverſy promiſes to be of no advantage to ſcience: I will therefore in future diſregard all remarks and objections which do not attack the fundamental principle of my theory.

JOHN GOUGH.

Middleſhaw near Kendal, Feb. 11, 1803.

III

Obſervations on the two lately diſcovered celeſtial Bodies. By WILLIAM HERSCHEL, *LL. D. F. R. S.*

(Concluded from Page 128.*)*

WITH regard to the fifth, concerning ſatellites, it may not be eaſy to prove a negative; though even that, as far as it can be done, has been ſhewn. But the retention of a ſatellite in its orbit, it is well known, requires a proper maſs of matter in the central body, which it is evident theſe ſtars do not contain.

The ſixth article ſeems to exclude theſe ſtars from the condition of planets. The ſmall comas which they ſhew, give them

them ſo far the reſemblance of comets, that in this reſpect we ſhould be rather inclined to rank them in that order, did other circumſtances permit us to aſſent to this idea.

In the ſeventh article, they are again unlike planets; for it appears, that their orbits are too near each other to agree with the general harmony that takes place among the reſt; perhaps one of them might be brought in, to fill up a ſeeming vacancy between Mars and Jupiter. There is a certain regularity in the arrangement of planetary orbits, which has been pointed out by a very intelligent aſtronomer, ſo long ago as the year 1772; but this, by the admiſſion of the two new ſtars into the order of planets, would be completely overturned; whereas, if they are of a different ſpecies, it may ſtill remain eſtabliſhed.

The new ſtars are not planets.

As we have now ſufficiently ſhewn that our new ſtars cannot be called planets, we proceed to compare them alſo with the other propoſed ſpecies of celeſtial bodies, namely, comets. The criteria by which we have hitherto diſtinguiſhed theſe from planets, may be enumerated as follows.

Criterions of comets.

1. They are ſmall.

1. They are celeſtial bodies, generally of a very ſmall ſize, though how far this may be limited, is yet unknown.

2. Orbits excentric.

2. They move in very excentric ellipſes, or apparently parabolic arches, round the ſun.

3. Planes various.

3. The planes of their motion admit of the greateſt variety in their ſituation.

4. Direction various.

4. The direction of their motion alſo is totally undetermined.

5. Great atmoſpheres.

5. They have atmoſpheres of very great extent, which ſhew themſelves in various forms of tails, coma, hazineſs, &c.

Compariſon of the new ſtars in theſe reſpects.

On caſting our eye over theſe diſtinguiſhing marks, it appears, that in the firſt point, relating to ſize, our new ſtars agree ſufficiently well; for the magnitude of comets is not only ſmall, but very unlimited. Mr. Pigott's comet, for inſtance, of the year 1781, ſeemed to have ſome kind of nucleus; though its magnitude was ſo ill defined, that I probably overrated it much, when, November 22, I gueſſed it might amount to 3 or 4″ in diameter. But, even this, conſidering its nearneſs to the earth, proves it to have been very ſmall.

That of the year 1783, alſo diſcovered by Mr. Pigott, I ſaw to more advantage, in the meridian, with a 20-feet reflector. It had a ſmall nucleus, which, November 29, was coarſely eſtimated to be of perhaps 3″ diameter. In all my other pretty

numerous

numerous obſervations of comets, it is expreſsly remarked, that they had none that could be ſeen. Beſides, what I have called a nucleus, would ſtill be far from what I now ſhould have meaſured as a diſk; to conſtitute which, a more determined outline is required.

In the ſecond article, their motions differ much from that of comets; for, ſo far as we have at preſent an account of the orbits of theſe new ſtars, they move in ellipſes which are not very excentric.

Nor are the ſituations of the planes of their orbits ſo much unlike thoſe of the planets, that we ſhould think it neceſſary to bring them under the third article of comets, which leaves them quite unlimited.

In the fourth article, relating to the direction of their motion, theſe ſtars agree with planets, rather than with comets.

The fifth article, which refers to the atmoſphere of comets, ſeems to point out theſe ſtars as belonging to that claſs; it will, however, on a more particular examination, appear that the difference is far too conſiderable to allow us to call them comets.

Account of the ſize of tails of comets.

The following account of the ſize of the comas of the ſmalleſt comets I have obſerved, will ſhew that they are beyond compariſon larger than thoſe of our new ſtars.

Nov. 22, 1781. Mr. Pigott's comet had a coma of 5 or 6′ in diameter.

Nov. 29, 1783. Another of Mr. Pigott's comets had a coma of 8′ in diameter.

Dec. 22, 1788. My ſiſter's comet had a coma of 5 or 6′ in diameter.

Jan. 9, 1790. Another of her comets was ſurrounded by hazineſs of 5 or 6′ in diameter.

Jan. 18, 1790. Mr. Mechain's comet had a coma of 5 or 6′ in diameter.

Nov. 7, 1795. My ſiſter's comet had a coma of 5 or 6′ in diameter.

Sept. 8, 1799. Mr. Stephen Lee's comet had a coma of not leſs than 10′ in diameter, and alſo a ſmall tail of 15′ in length.

From theſe obſervations, which give us the dimenſions of the comas of the ſmalleſt comets that have been obſerved with good inſtruments, we conclude, that the comas of theſe new ſtars, which at moſt amount only to a few times the diameter of

of the bodies to which they belong, bear no resemblance to the comas of comets, which, even when smallest, exceed theirs above a hundred times. Not to mention the extensive atmospheres, and astonishing length of the tails, of some comets that have been observed, to which these new stars have nothing in the least similar.

Since, therefore, neither the appellation of planets, nor that of comets, can with any propriety of language be given to these two stars, we ought to distinguish them by a new name, denoting a species of celestial bodies hitherto unknown to us, but which the interesting discoveries of Mr. Piazzi and Dr. Olbers have brought to light.

These new stars cannot with propriety of language be called planets or comets.

With this intention, therefore, I have endeavoured to find out a leading feature in the character of these new stars; and, as planets are distinguished from the fixed stars by their visible change of situation in the zodiac, and comets by their remarkable comas, so the quality in which these objects differ considerably from the two former species, is that they resemble small stars so much as hardly to be distinguished from them, even by very good telescopes. It is owing to this very circumstance, that they have been so long concealed from our view. From this, their asteroidical appearance, if I may use that expression, therefore, I shall take my name, and call them *Asteroids*: reserving to myself, however, the liberty of changing that name, if another, more expressive of their nature, should occur. These bodies will hold a middle rank, between the two species that were known before; so that planets, asteroids, and comets, will in future comprehend all the primary celestial bodies that either remain with, or only occasionally visit, our solar system.

The author proposes to call them *Asteroids*.

I shall now give a definition of our new astronomical term, which ought to be considerably extensive, that it may not only take in the asteroid Ceres, as well as the asteroid Pallas, but that any other asteroid which may hereafter be discovered, let its motion or situation be whatever it may, shall also be fully delineated by it. This will stand as follows.

Asteroids are celestial bodies, which move in orbits either of little or of considerable excentricity round the sun, the plane of which may be inclined to the ecliptic in any angle whatsoever. Their motion may be direct, or retrograde; and they may or may not have considerable atmospheres, very small comas, disks, or nuclei.

Definition of asteroids.

Others will ſoon be found out;

As I have given a definition which is ſufficiently extenſive to take in future diſcoveries, it may be proper to ſtate the reaſons we have for expecting that additional aſteroids may probably be ſoon found out. From the appearance of Ceres and Pallas it is evident, that the diſcovery of aſteroids requires a particular method of examining the heavens, which hitherto aſtronomers have not been in the habit of uſing. I have already made five reviews of the zodiac, without detecting any of theſe concealed objects. Had they been leſs reſembling the ſmall ſtars of the heavens, I muſt have diſcovered them. But the method which will now be put in practice, will completely obviate all difficulty ariſing from the aſteroidical appearance of theſe objects; as their motion, and not their appearance, will in future be the mark to which the attention of obſervers will be directed.

by obſerving the relative motion as well as the appearance of ſtars.

Society for that purpoſe.

A laudable zeal has induced a ſet of gentlemen on the Continent, to form an aſſociation for the examination of the zodiac. I hope they will extend their attention, by degrees, to every part of the heavens; and that the honourable diſtinction which is juſtly due to the ſucceſsful inveſtigators of nature, will induce many to join in the meritorious purſuit. As the new method of obſerving the zodiac has already produced ſuch intereſting diſcoveries, we have reaſon to believe that a number of aſteroids may remain concealed; for, how improbable it would be, that if there were but two, they ſhould have been ſo near together as almoſt to force themſelves to our notice. But a more extended conſideration adds to the probability that many of them may ſoon be diſcovered. It is well known that the comas and tails of comets gradually increaſe in their approach to the ſun, and contract again when they retire into the diſtant regions of ſpace. Hence we have reaſon to expect, that when comets have been a conſiderable time in retirement, their comas may ſubſide, if not intirely, at leaſt ſufficiently to make them aſſume the reſemblance of ſtars; that is, to become aſteroids, in which ſtate we have a good chance to detect them. It is true that comets ſoon grow ſo faint, in retiring from their perihelia, that we loſe ſight of them; but, if their comas, which are generally of great extent, ſhould be compreſſed into a ſpace ſo ſmall as the diameters of our two aſteroids, we can hardly entertain a doubt but that they would again become viſible with good teleſcopes. Now, ſhould we ſee a comet in its aphelion, under the conditions here pointed out, and that there are many which may be in ſuch ſituations, we have the greateſt

Comets may become aſteroids;

and be obſerved in their aphe-

greateſt inducements to believe, it would be a favourable circumſtance to lead us to a more perfect knowledge of the nature of comets and their orbits; for inſtance, the comet of the year 1770, which Mr. Lexell has ſhewn to have moved in an elliptical orbit, ſuch as would make the time of its periodical return only about $5\frac{1}{2}$ years: if this ſhould ſtill remain in our ſyſtem, which is however doubtful, we ought to look for it under the form of an aſteroid.

If theſe conſiderations ſhould be admitted, it might be objected, that aſteroids were only comets in diſguiſe; but, if we were to allow that comets, aſteroids, and even planets, might poſſibly be the ſame ſort of celeſtial bodies under different circumſtances, the neceſſary diſtinction ariſing from ſuch difference, would fully authoriſe us to call them by different names.

It is to be hoped that time will ſoon throw a greater light upon this ſubject; for which reaſon, it would be premature to add any other remarks, though many extenſive views relating to the ſolar ſyſtem might certainly be hinted at.

Additional Obſervations relating to the Appearances of the Aſteroids Ceres and Pallas.

Additional obſervations.

The comas do not much exceed thoſe of the fixed ſtars.

May 4, $12^h\ 40'$. 10-feet reflector; power $516\frac{1}{2}$. I compared Ceres with two fixed ſtars, which, in the finder, appeared to be of very nearly the ſame magnitude with the aſteroid, and found that its coma exceeds their aberration but in a very ſmall degree.

$12^h\ 50'$. 20-feet reflector; power 477. I viewed Ceres, in order to compare its appearance with regard to hazineſs, aberration, atmoſphere, or coma, whatever we may call it, to the ſame phenomena of the fixed ſtars; and found that the coma of the aſteroid did not much exceed that of the ſtars.

I alſo found, that even the fixed ſtars differ conſiderably in this reſpect among themſelves. The ſmaller they are, the larger in proportion will the attendant hazineſs ſhew itſelf. A ſtar that is ſcarcely perceptible, becomes a ſmall nebuloſity.

10-feet reflector. $13^h\ 10'$. I compared the appearance of Pallas with two equal fixed ſtars; and found that the coma of this aſteroid but very little exceeds the aberration of the ſtars.

$14^h\ 5'$, 20-feet reflector. I viewed Pallas; and, with a magnifying power of 477, its diſk was viſible. The coma of this aſteroid is a little ſtronger than that which fixed ſtars of the ſame ſize generally have.

On

IV.

On the oblique Refraction of Iceland Cryſtal. *By* WILLIAM HYDE WOLLASTON, *M. D. F. R. S.* *From the Phil. Tranſ. for* 1802.

IN the preceding communication *, I have inſerted two different meaſures of refractive powers, diſtinctly obſervable in the Iceland cryſtal, as well as an eſtimate of its diſperſive power; but have reſerved for a ſeparate treatiſe, ſome renarks which the ſame mode of inveſtigation has enabled me to make on its oblique refraction.

Iceland cryſtal well examined by Huygens.

The optical properties of this body have been ſo amply deſcribed by Huygens in his *Traité de la Lumière,* that it could anſwer little purpoſe to attempt to make any addition to thoſe which he has enumerated. But, as the law to which he has reduced the oblique refractions occaſioned by it, could not be verified by former methods of meaſurement, without conſiderable difficulty, it may be worth while to offer a new and eaſy proof of the juſtneſs of his concluſions. For, ſince the theory by which he was guided in his inquiries. affords (as has lately been ſhown by Dr. Young †) a ſimple explanation of ſeveral phenomena not yet accounted for by any other hypotheſis, it muſt be admitted that it is entitled to a higher degree of conſideration than it has in general received.

His theory,

that light is propagated by vibrations of a medium; undulating ſpherically in moſt caſes; but in an oblate ſpheroid, in Iceland cryſtal:

According to that hypotheſis, light proceeding from any luminous centre, is propagated by vibrations of a medium highly elaſtic, that pervades all ſpace. In ordinary caſes, the incipient undulations are of a ſpherical form; but, in the Iceland cryſtal, light appeared to Huygens to proceed as if the undulations were portions of an oblate ſpheroid, of which the axis is parallel to the ſhort diagonal of an equilateral piece of the cryſtal and its centre the point of incidence of the ray.

Whence the law of this refraction: viz. that the ſine of incidence has a conſtant ratio to the ordinate of refraction in the ſpheroid.

From this ſpheroidical form of the undulations, he deduces the obliquity of refraction; and lays down a law, obſervable in all refractions, at any ſurface of the ſpar, whether natural or artificial, which bears the cloſeſt analogy to that which obtains univerſally at other refracting ſurfaces; for as, in other

* See our Journal IV. 89.

† Bakerian Lecture. Phil. Trans. for 1801.

caſes,

cafes, the ratio is given between the fine of incidence and fine of refraction, (or ordinate of the *fpherical* undulation propagated,) fo, in the Iceland cryftal, the ratio between the fine of incidence and ordinate of refraction (in any one fection of the *fperoidical* undulation) is a given ratio.

If ABD (Fig. 1, Plate VIII.) be any furface of the fpar, let FHOK be a fection of the fpheroid through its centre C, and RC any ray of light falling on that furface; draw FO a diameter of the fpheroid, in the plane of incidence RVO, and CT, its femiconjugate diameter, in the plane of refraction FTO. Then, if CI be the refracted ray, VR, the fine of incidence, fhall be to EI, the ordinate of refraction parallel to FC, in the conftant ratio of a given line N to the femidiameter FC. Illuftration by diagram.

In any other plane of incidence, the ratio of fine to ordinate is alfo conftant; but it is a different ratio, according to the magnitude of that diameter in which the plane of incidence interfects the ellipfe FHOK. But the refraction will differ, in different planes of incidence, as the diameter of the ellipfe, &c.

When the incidence of the ray paffing from any medium of greater denfity upon the furface of this fpar, is fuch that the emergent ray becomes parallel to the furface, the ordinate of refraction is then a femidiameter of the fpheroid; and, accordingly, the refractive power of this fpar, when examined by means of a prifm in different directions, fhould be found to vary as that femidiameter which coincides with the plane of incidence and refracting furface. as may be feen by the angle of total reflection.

The obfervations that I have made on this fubftance, accord throughout with this hypothefis of Huygens; the meafures that I have taken correfpond more nearly than could well happen to a falfe theory, and are the more to be depended on, as all my experiments, excepting the laft, were made prior to my acquaintance with the theory, and their agreement was deduced by fubfequent computation. Hypothefis of Huygens confirmed.

Exp. 1. The oblique refraction of this fpar is rendered vifible, by cementing a furface of it to a prifm of flint-glafs, with a little balfam of Tolu. When the line of fight befects an acute angle of a natural furface of the fpar, the refractive power is feen to be lefs than in any other direction, and may be expreffed by the fine 1,488, or its reciprocal 0,67204. Experiments on the oblique refraction in different planes.

Exp. 2. When the plane of incidence is parallel to one of the fides, the power is 1,518, of which the reciprocal is 0,6587.

Exp.

Exp. 3. In a direction at right angles with either ſide, it is found ſtill higher, being 1,537, or its reciprocal 0,6506.

Exp. 4. And in the plane biſecting an obtuſe angle, the refractive power of the natural ſurface appears greater, and is expreſſed by the ſine 1,571, or its reciprocal 0,6365.

and ſurfaces.

Exp. 5. When either of the two greateſt ſolid angles of the ſpar contained under three obtuſe angles, is cut off by a poliſhed ſurface making equal angles with each of its ſides, the ſame refractive power 1,488 is found in all directions. By the theory alſo, the ſection of the ſpheroid is in this caſe a circle, and every ſemidiameter (FC) the ſame, ſince the plane is at right angles to the minor axis.

Exp. 6. If a plane ſurface be formed biſecting an obtuſe angle of the ſpar, and applied to a priſm, the ſame minimum of refraction 1,488, is found in a direction that coincides with the preceding plane, and therefore with the major axis of the generating ellipſe; but, as the direction is varied, it increaſes ſo rapidly as ſoon to exceed the power of glaſs, and to be no longer aſcertainable by the angle of incipient reflection.

Regular refraction.

Exp. 7. The regular refraction of this ſpar is alſo too great for examination by means of any priſm, for want of a medium of union of ſufficient denſity; but, by trial in the uſual method, it meaſured, on an average of ſeveral experiments, 1,657, or its reciprocal 0,6035.

Spheroid which regulates the refractions.

By aſſuming, as Huygens has done, the equality of this power with the maximum of the oblique refraction, we have ſufficient data for conſtruction of the ſpheroid by which the refractions are regulated; for we have 0,67204 (*Exp.* 1.) as major axis of the generating ellipſe, and 0,6035 (*Exp.* 7) will be the minor axis, parallel in poſition to the ſhort axis of the ſpar.

The angle of inclination of this axis to the ſurfaces of the ſpar, if ſuppoſed to be equilateral, may be computed by ſpherical trigonometry, from any other angle that has been aſcertained by meaſurement.

The meaſures that I have taken are not exactly thoſe of Huygens; but I nevertheleſs hold them in equal eſtimation, from the conformity which I find they bear to each other, by aſſiſtance of his theory.

Deduction of its dimenſions and poſition.

Exp. 8. I meaſured with care an angle at which two ſurfaces of the ſpar are inclined to each other, and found it to be 105° 5′.

Hence

Hence, the greater angle of the ſurfaces themſelves may be computed to be 101° 55′; and the angle which the ſhort axis makes with each plane ſurface is 45° 23′ 25″.

If GSMP (Fig. 2.) be a plane biſecting an obtuſe angle of the ſpar, the ſection of the ſpheroid in that plane paſſes through the axis CS, and therefore is the generating ellipſe. By calculating from the known dimenſions of its major axis CP 0,67204, its minor axis CS 0,6035, and the angle GCS=45° 23′ 25″, CG will be found * to be 0,6365, of which the reciprocal is 1,5736, differing but little from 1,571, as it appeared by meaſurement. (*Exp.* 4.)

Again, if ABDE (Fig. 4.) be one of the natural ſurfaces, and PGp the ellipſe formed by that ſection of the ſpheroid, PC being as before 0,67204, and CG 0,6365, the reciprocal of 1,571 found by meaſurement, (*Exp.* 4) then the ſemidiameter CT, parallel to the ſide AE, which makes an angle TCP 39° 2½′, will be found to be 0,6573, inſtead of 0,6587, and its reciprocal 1,5215, inſtead of 1,518. (*Exp.* 2.)

The ſemidiameter alſo, in the direction of CL, perpendicular to the ſide, at an angle LCP 50° 57½′, is found by calculation 0,650, and its reciprocal 1,539, inſtead of 0,6506 and 1,537. (*Exp.* 3.)

Courſe of a perpendicular ray after this refraction.

From the foregoing data, the courſe of a ray perpendicular to the ſurface of the ſpar may likewiſe be computed; for, ſince the ſine of incidence is then nothing, the ordinate of refraction muſt be alſo nothing, and the ray will be refracted along the ſemiconjugate diameter CM. (Fig. 2.)

By calculation †, the angle which this conjugate makes with the perpendicular is 6° 7½′. But, by the following meaſurement, it appears to be 6° 16′.

Experiment to ſhew the ſame courſe.

Exp. 9. A piece of ſpar that meaſured 1,145 inch in thickneſs, was laid upon a line, and ſhowed two images that were removed from each other $\frac{126}{1000}$ of an inch. Then, as 1,145 : 0,126 :: radius : tang. of 6° 16′.

Comparative ſtatement of reſults from obſervation and from theory.

The different reſults deduced from theory and from obſervation, will be ſeen at one view in the following ſtatement:

* (Fig. 3.) CS : CP :: tang. PCG : tang. PCp ſec. PCp : ſec. PCG :: CP : CG.

† (Fig. 5.) CS : CP :: tang. PCG : tang. pCO or co-tang. PCQ; then CP : CS :: tang. PCQ : tang. PCM; and LCP—PCM=MCL.

In

In *Exp.* 2d,	observed	1,518;	calculated	1,5215.
3d,	———	1,537	———	1,539.
4th,	———	1,571	———	1,5736.
6th, angle observed		6° 16′	- -	6° 7½′.

Remarks.

The angle observed differs from that obtained by computation, in a greater degree than any of the former measures; but, when the difficulty of measuring this angle with accuracy is considered, and also the greater effect of any incorrectness in the data from which a semiconjugate is computed, I think the result of this, as well of the preceding comparisons, must be admitted to be highly favourable to the Huygenian theory; and, although the existence of two refractions at the same time, in the same substance, be not well accounted for, and still less their interchange with each other, when a ray of light is made to pass through a second piece of spar situated transversely to the first, yet the oblique refraction, when considered alone, seems nearly as well explained as any other optical phenomenon.

V.

The Theory of Compound Sounds, By Mr. John Gough *.

Dr. Smith's theory of compound sounds; that the pulses do not obstruct or become confounded with each other.

Dr. SMITH, author of the work on Harmonics, takes for granted in his theory of compound sounds, that the pulses which proceed at the same time from a number of sounding bodies, do not clash, or obstruct one another, in their passage through the air. According to this hypothesis, each set, of any number of cotemporary sets of pulses, strikes the ear without being confounded with the rest; in consequence of which, any number of sounds may be distinctly perceived at the same time. On this supposition, a compound sound is a sensation rendered variable by the irregular manner in which the pulses of the constituent sounds succeed to one another. For, if the intervals of time between two successive pulses of

* Manchester Memoirs, v. 653. This treatise which was not inserted at an earlier period in our Journal, on account of the pressure of other matter, has become of still more interest as containing the theory to which the author's late discussion with Dr. Young is directed.—W. N.

one

one of the conſtituent ſounds be not equal to the ſame intervals belonging to the ſound or ſounds which accompany it, the ſecondary intervals, or ſmall parts of time ſeparating the pulſes which fall in ſucceſſion on the ear, will vary in magnitude; in the ſame manner that the diſtances between the figures upon the face of a barometer and its nonius vary, none on the ſlider coinciding with thoſe on the fixed plate excepting the higheſt and loweſt. I have choſen this familiar inſtrument to illuſtrate Dr. Smith's method of explaining the phyſical cauſe of compound ſounds, becauſe it affords a viſible example of a cycle of pulſes, according to his notion of the ſubject.

The intervals of the pulſes are not equal, but increaſe and diminiſh periodically, as in a ſcale and nonius.

The ſketch which I have exhibited of Dr. Smith's hypotheſis ſhews, that he allowed that a number of ſimple ſounds might exiſt in concert, and ſtrike the ear in a diſtinct manner, without ſuffering any interruption in their motions from the interference of their pulſes. But a late writer on ſound rejects this axiom in Harmonics as a mathematical inconſiſtency; and ſubſtitutes the following theory of compound ſounds in the room of it. If two muſical ſtrings, differing in their times of vibration, happen to vibrate in concert, they do not occaſion two diſtinct ſounds in the opinion of this gentleman, becauſe the ſtrings agitate the air in conjunction; conſequently the pulſes, which one of them would actually form in an undiſturbed atmoſphere, muſt unavoidably claſh with thoſe which the other ſtring would produce in ſimilar circumſtances. Hence the waves of air belonging to both ſtrings are interrupted in their natural progreſs, and are compelled by their mutual interference to coaleſce, thereby producing a new ſucceſſion of pulſes, conſtituting a ſingle ſound in the place of two. This ſound is of a peculiar kind; for, the pulſes of which it conſiſts, are ſeparated by unequal intervals of time, and diſpoſed in cycles.

Another theory (Dr. Young's), that the pulſes coaleſce; and form a peculiar ſound, having its pulſes unequal and diſpoſed in cycles.

The merit of the preceding theory, when compared with Dr. Smith's hypotheſis, muſt be aſcertained by contraſting it with a variety of facts, which are furniſhed by the phænomena of compound ſounds, and make a part of every man's experience. For, if it be found upon examination to be repugnant to theſe facts, it will prove inconſiſtent with nature, and cannot fail of diſappointing the inventor's expectations.

Examination of the latter theory.

Were it poſſible for a number of ſounds to coaleſce, and form but one, the compound would acquire ſenſible properties peculiar

If ſounds could coaleſce they would form a new compound;

to itſelf, and at the ſame time loſe the diſtinguiſhing characters of its elements, ſome of which are incompatible with the qualities of an individual. On this ſuppoſition, the preſence of the conſtituent ſounds could not be detected by the ear in this newly created being: on the contrary, an experimental proceſs would be required to analyſe every compound ſound the firſt time it attracted a man's attention, for the ſame reaſon that a chemiſt finds it neceſſary to analyſe a ſubſtance with which he is unacquainted. The abſtract term *coaleſcence* is uſed, in a phyſical ſenſe, to ſignify any intimate union of bodies or the powers of bodies; and the introduction of the term into language proves the exiſtence of the principle in nature, or more properly in the human mind. For, when a number of agents act in conjunction upon one of the ſenſes, we have two ways of conceiving their mode of operation. If the ſenſible effects of each agent be diſtinctly perceived, we attribute a ſeparate action to every member of the aſſemblage, and call the aggregate a mixture: this is the concluſion of a perſon who taſtes an infuſion of pepper in vinegar. On the other hand, when we know that certain agents are preſent without being able to recognize their diſtinguiſhing powers, in the room of which we find qualities of a different deſcription, we pronounce the aggregate to be in a ſtate of coaleſcence. This is the ſituation of the chemiſt, who taſtes common ſalt, but cannot perceive the preſence of ſoda and the muriatic acid. It is my buſineſs then to prove compound ſounds to be *mixtures*, not aggregates by coaleſcence. This I ſhall endeavour to do, by ſhewing that they have properties which belong not to individuals, ſuch as a number of tones, a variety of directions, and ſeveral ſets of pulſes.

in which the ear could not diſtinguiſh the component parts.

Chemical compariſon.

Compound ſounds are mixtures; not combinations.

Firſt, the tones of a flute and violin are as diſtinct to ſenſe as any two things can be when they are ſounded ſeparately; and I appeal to common experience to determine, if they are not equally diſtinct when heard in concert. Taking it for granted that the anſwer will be in the affirmative, I pronounce the aggregate to be a mixture of ſounds in one caſe. Secondly, if a violin ſound in front of the hearer, and a flute be heard at the ſame time in an oblique ſituation, the perſon thus circumſtanced is able to determine the relative poſitions of the two inſtruments, which ſhews the aggregate to have two contemporary directions. It is therefore a mixture of ſounds, not a ſingle

Inſtances. The flute and violin perfectly diſtinguiſhable in concert,

the courſes of the two ſounds are diſtinguiſhed when they differ.

a ſingle ſound. Thirdly, I have found by making the experiment, that any number of muſical ſtrings may be made to vibrate by a compound ſound acting upon them, provided this compound be occaſioned by an equal number of ſtrings with the former, having one in the latter ſet in uniſon with each one in the preceding ſet. This is an experimental proof that there are as many ſets of pulſes in an aggregate of ſounds as that aggregate contains elements, becauſe no ſtring whatever is in uniſon with a concord or diſcord. Laſtly, if it were poſſible for ſounds to coaleſce, men would never hear any thing more than one noiſe at one time: The general hum would have varied perpetually from the extinction of exiſting ſounds, and the intruſion of freſh ones; but the human mind would have had no conception of two cotemporary ſounds; becauſe the ear being in that caſe incapable of conveying the complex ſenſation, the idea of ſuch an exiſtence would have tranſgreſſed the ſphere of human knowledge. The preceding arguments are drawn, for the moſt part, from common experience; and they ſhew, that the free paſſage of cotemporary ſounds through the air may be ſafely admitted as an axiom in harmonies. I ſhall therefore proceed to prove the ſame propoſition to be conſiſtent with the doctrine of forces.

A number of ſtrings will be ſet into vibration by the compound ſound of other ſtrings giving uniſon to each reſpectively.

If ſounds coaleſce we ſhould conſtantly hear one varying noiſe.

The propagation of ſound through the atmoſphere, and the nature of aëreal pulſes are commonly explained in elementary books of natural philoſophy; I ſhall, for this reaſon, enumerate only a few particulars, the recollection of which will be found uſeful.

Mechanical arguments.

Propoſition I. Two contiguous particles of air which are agitated by a vibrating body, either directly or by the intervention of an elaſtic medium, receive two motions from each impulſe; *firſt*, an abſolute motion carries them to a greater diſtance from the ſounding body, and afterwards brings them towards it again, both the progreſs and regreſs being performed in the time of a ſingle vibration: *ſecond*, a relative motion reſulting from the former, compels the two particles to approach and recede alternately, which double motion is alſo accompliſhed in the time of a ſingle vibration.

Prop. 1. Vibration of the particles of air, in ſound.

Propoſition II. Both the abſolute and relative motions are greateſt amongſt thoſe particles which are neareſt the ſounding body, and they diminiſh as the diſtance from that body increaſes; but, in all caſes, the change of its place too ſmall to be perceived

Prop. 2. The particles are not ſenſibly removed.

perceived by the ear, on which account every particle preſerves a fixed poſition in reſpect of this organ and its connections.

Phyſical right line.

For each corpuſcle is confined within the circumference of a phyſical right line, the diameter of which is determined by its own abſolute motion.

Prop. 3. A particle in the interſection of two rows of particles conveying different ſounds, will be urged in thoſe lines by forces varying in their ratio.

Propoſition III. If two ſounding bodies, affording different notes, act in conjunction upon the ſame particle, through the media of two right lines of ſimilar particles connecting them with it, this particle will be urged at the ſame inſtant in the direction of theſe lines, by two forces having an inconſtant ratio.

For, let the particle A be urged, by the acuter ſound, in the line SA, and by the graver, in the line TA; *(vide* Fig. 2, Plate IX.); then the contiguous particle V, placed in SA, will approach to, and recede from A more frequently than W, ſimilarly placed in TA, by Prop. I.: conſequently the force of V upon A will vary in a quicker manner than the force of W upon A; but this variation of ratio is limited in time; becauſe it evidently begins and ends with the cycle of the vibrations of the ſounding bodies.

Prop. 4. The right lined motion of the particle neceſſary to form a ſound by coaleſcence is impoſſible.

Propoſition IV. The coaleſcence of two ſounds is impoſſible on mechanical principles.

For, ſuppoſe the thing poſſible; then the coaleſcence of two ſounds requires, that a particle of air ſhould poſſeſs a motion, compounded of the motions which the two ſounding bodies would impart to it ſeparately; and that this compound motion ſhould act in a given right line, for an aſſignable part of time, otherwiſe it could not excite a ſimilar motion in the elaſtic particles occupying that given right line. Let A be ſuch a particle, and let the conſtruction uſed in the laſt propoſition, be retained; conſequently (Principia, Prop. 23, Lib. 2.) VA and AW are in the ratio of the forces that act at any moment in the right lines TA and SA. Make AK as AW, and draw KL parallel to AW, and make it as AV; alſo join AL; then will the particle A be urged in the direction LA at that inſtant. But the ratio of AK to KL varies perpetually, by Prop. III.; therefore the ſpecies of the triangle AKL is equally inconſtant; conſequently the compound force does not act in a given direction for an aſſignable part of time. Now the production and propagation of motion in a given right line requires force

to

to be combined with time, which combination is wanting in the prefent inftance; wherefore the coalefcence of founds is impoffible.

Propofition V. It may be demonftrated from mechanical principles, that a number of diftinct cotemporary founds cannot do otherwife than produce diftinct fenfations.

Prop. 5. Diftinct cotemporary founds produce diftinct fenfations.

In order to make the neceffary diagram as fimple as poffible, let the directions of two cotemporary fets of pulfes be reprefented by the right lines SM and TN, lying in the fame horizontal plane, and interfecting in the point A; alfo, let BCD be the horizontal fection of the hearer's head, made by the fame plane; and fuppofe the centre of the axis of hearing to be at O; draw OM, ON perpendicular to SM, TN. Now I have fhewn in the preceding paper*, that if a fet of pulfes move in either of the right lines SM, TN, it will excite a fenfation in that part of the head which is cut off by a vertical plane, paffing through one of the perpendiculars OM, ON. It alfo appears from the laft propofition, that the impulfes of the vibrating bodies, acting in the lines SA, TA, do not compel the particle A to move in any given intermediate direction, as LA. But, according to the fecond propofition, the pofition of the particle A, is fixed in refpect of the planes MO, NO; that is, though the corpufcle actually changes place, in refpect of the geometrical point A, it is always found in the interfection of the phyfical right lines SM, TN. Now the two vibrating bodies continue to act in the directions of thefe right lines, confequently the particle A is conftantly urged in thefe lines by two forces, which, though variable in magnitude, are combined with time; which circumftance enables the corpufcle to tranfmit the impulfes of one body to M, and thofe of the other to N. What has been demonftrated of the particle A, may be affirmed of any other particle, which is the interfection of two right lines parallel to SM, TN; in other words, it may be affirmed of two fets of pulfes; and the fame demonftration may be extended to three fets, &c.

Corollary 1. The fubftance of this and the preceding propofition will apply to all elaftic mediums; hence it happens, that a plate of glafs, &c. in a ftate of vibration, will conduct a foreign found, whilft it produces one of its own; for the

Thefe propofitions apply to founds conducted through folids, &c.

* Manchefter Mem. v. 342, or Philof. Journal, II. 460.

fame

fame reafon, if light be confidered as a vibrating medium, one particle of the luminous fluid may affift in tranfmitting two fenfations.

The directions will not be perceived unlefs they differ by a certain angle.

Corollary 2. When the inclination of the planes MO, NO, is lefs than a given angle, the ear cannot diftinguifh the relative pofitions of the founding bodies; it therefore refers them to the fame place.

Interval of found.

Analyfis of the human voice.

The firft time I perufed Dr. Smith's Harmonics, Dr. Young's objection occurred to me; but the preceding train of arguments removed the fcruple, without difcovering the author's reafons for treating this article of his work with fo much brevity. Perhaps the demonftration, which coft me an effort of ftudy, was an intuitive conclufion in his comprehenfive mind. As foon as the propofition was eftablifhed, I affented to his definition of an interval of found, allowing it to be a quantity of a certain kind, terminated by a graver and an acuter found. The demonftration of Prop. V. convinced me, that intervals of this fort may be fubdivided by the interpofition of one or more intermediate founds, which conceffion formed the bafis of my analyfis of the human voice.* Speculative men may differ in opinion about the origin of the fmall intervals which form the tones of various voices; but they muft exift, whether we afcribe them to an undulating motion like that of a ftretched cord, or to the cotemporary vibrations of a fyftem of elaftic bodies. It does not appear, that Dr. Young was acquainted with my paper at the time he compofed his own; but he found it neceffary to allow the tone of the larynx to receive various modifications from the vibrations of the adjacent parts. His theory therefore differs from mine in this particular only: he pronounces the voice to be a compound by coalefcence; I deny the poffiblility of fuch a compound, and call it a mixture of imperfect unifons. This mixture appears to be a fingle found, becaufe it has but one direction; for the proximity of the various parts contributing to the formation of it, difqualifies the ear, fo that it cannot perceive their relative pofitions, and compels it to refer them all to one place, by Corollary 2 to propofition V.

* Manchefter Mem. v. 58, or Phil. Journal, Quarto, IV. 46. for an abridged ftatement.

A certain

A certain clafs of founds, which, for the fake of brevity, were not noticed in my paper on the voice, deferve a place in the prefent communication. If a finely-toothed file pafs flowly over a fmooth elaftic fubftance, fuch as a piece of horn, it makes a grating noife; but if the velocity of the inftrument be fufficiently increafed, a continued found is produced, which becomes more or lefs acute, by giving a quicker or flower motion to the file. The grating noife is occafioned by a fucceffion of fhort interrupted founds, refulting from the united vibrations of the file and the body it fcratches; but the quick fucceffion of thefe founds, caufed by an increafe of velocity, gives rife to a fecondary found refembling the harmonical notes, being produced by a like caufe. Now this found becomes a primary object with the ear, in all probability becaufe the pitch of it may be varied; for the firft founds proceeding from the action of the file, evidently fupply nothing but the tone. Many inftances of the kind occur in art and nature: the notes of all reed-inftruments are of this defcription, and the voice muft be referred to the fame clafs, becaufe the larynx refembles a reed-inftrument in ftructure.

Clafs of founds produced by the quick fucceffion of other founds; the firft giving the tune or pitch, and the latter the tone or character.

VI.

Experiments and Obfervations to determine whether the Quantity of Rain and Dew is equal to the Quantity of Water carried off by the Rivers and raifed by Evaporation; with an Enquiry into the Origin of Springs. By Mr. John Dalton *.

It is fcarcely poffible to contemplate without admiration the beautiful fyftem of nature by which the furface of the earth is continually fupplied with water, and that unceafing circulation of a fluid fo effentially neceffary to the very being of the animal and vegetable kingdoms takes place. Naturalifts, however, are not unanimous in their opinions whether the rain that falls is fufficient to fupply the demands of fprings and rivers, and to afford the earth befides fuch a large portion for evaporation as it is well known is raifed daily. To afcertain this point is an object of importance to the fcience of agriculture, and to every concern in which the procuration and

Interefting fyftem by which the globe is fupplied with water.

Whether the rain be equal to all the demands of rivers, &c.

* Manchefter Memoirs, V. 346.

management

management of water makes a part, whether for domeſtic purpoſes or for the arts and manufactures.

For the ſake of perſpicuity I have diſtributed the ſubject under four heads:

Diviſion of the ſubject.

1. Of the quantity of rain and dew.
2. Of the quantity of water that flows into the ſea.
3. Of the quantity of water raiſed by evaporation.
4. of the origin of ſprings.

SECTION I.

An Eſtimate of the Quantity of Rain and Dew that falls in England and Wales in a Year.

Annual quantity of rain and dew in England and Wales. Inland counties have leſs rain than coaſts; and mountainous diſtricts have moſt.

Rain-gages have been fixed of late years in almoſt every part of the kingdom; by means of them we are enabled to determine, with conſiderable exactneſs, the depth of water that the rain yields in any given place. Inland counties have leſs rain than maritime ones, eſpecially thoſe which border on the weſtern ſeas. But a ſtill greater difference ſeems to take place between a mountainous country and a champaigne, or flat country: in the former there often falls double or triple the quantity of rain in a year, that there does in the latter, and never leſs than an equal quantity. It may be obſerved, that ſeveral years account of the rain at any place is required before a medium yearly quantity can be obtained with ſufficient accuracy. The following is perhaps the largeſt collection of accounts of rain fallen in different places in England that has hitherto appeared: They are moſtly taken from the Tranſactions of the Royal and other Societies.

Quantities tabulated.

Counties (maritime)	*Places.*	Mean annual depth in inches.
CUMBERLAND - -	Keſwick, 7 years - -	67. 5
	Carliſle, 1 year - - -	20. 2
WESTMORLAND -	Kendal, 11 years - - -	59. 8
	Fell-foot, 3 years - -	55. 7
	Waith Sutton, 5 years -	46
LANCASHIRE - -	Lancaſter, 10 years - -	45
	Liverpool, 18 years - -	34. 4
	Mancheſter, 9 years - -	33
	Townley - - -	41.
	Crawſhawbooth, near Haſlingden, 2 years - - - -	60

GLOUCESTERSHIRE

Counties (maritime)	*Places.*	Mean annual depth in inches.
GLOUCESTERSHIRE	Briſtol, 3 years - -	29. 2
SOMERSETSHIRE -	Bridgewater, 3 years - -	29. 3
CORNWALL - - -	Ludguan, near Mount's Bay, 5 years - - - - - - -	41
	Another place, 1 year - -	29. 9
DEVONSHIRE - -	Plymouth, 2 years - - -	46. 5
HAMPSHIRE - - -	Selbourne, 9 years - - -	37. 2
	Fyfield, 7 years - - - -	25. 9
KENT - - - - - -	Dover, 5 years - - - -	37. 5
ESSEX - - - -	Upminſter, - - - - - -	19. 5
NORFOLK - - - - -	Norwich, 13 years - - -	25. 5
YORKSHIRE - - -	Barrowby, near Leeds, 6 years	27. 5
	Garfdale, near Sedbergh, 3 y.	52. 3
NORTHUMBERLAND	Widdrington, 1 year - - -	21. 2

Counties (inland)	*Places.*	*Means.*
MIDDLESEX - - -	London, 7 years - - - - -	23
SURREY - - - -	South Lambeth, 9 years - -	22. 7
HERTFORDSHIRE -	Near Ware, 5 years - -	25
HUNTINGDONSHIRE	Kimbolton, 7 years - - -	25
DERBYSHIRE - -	Chatſworth, 15 years - -	27. 8
RUTLANDSHIRE -	Lyndon, 21 years - - - - -	24. 3
NORTHAMPTONSHIRE	Near Oundle, 14 years - -	23
	General Mean - -	35. 2

This general mean of 35. 2 inches is, I apprehend, a little above the medium for England and Wales, as the greater number of places are thoſe where much rain falls. If we take a mean for each of the above-mentioned counties (where more than one place in a county is given) and then a general mean from the counties, the reſult is a reduced mean of 31. 3. Even then it may be objected that the greater part of the counties are maritime; but it muſt be obſerved, that there is no account of rain in Wales; and we may ſafely conclude, that the rain in Wales would exceed the laſt-mentioned mean as

Mean quantity of rain.

much as the inland counties of England, not in the above list, would fall short; because Wales is both a mountainous country, and exposed to the sea.

Corrected is 31 inches for England and Wales.

We will therefore conclude, that the mean annual depth of rain in England and Wales, deduced from these 20 counties, is 31 inches: A quantity which subsequent observations, I am confident, will not diminish, and probably not increase much *.

Quantity of dew. It is water deposited by the cold of night.

It remains to estimate the quantity of dew that falls in a year. Some have doubted whether dew is derived from the air or the earth; but a proper attention to the phenomena will satisfy us, that it is a deposition of water, evaporated during the heat of the day. With respect to the quantity that falls in a year, we are much at a loss, as no daily observations have been made for a series of time that I know of: indeed, it would be difficult to prescribe a mode of observation. Dr. Hales † relates some experiments made to determine the quantity of dew that falls upon moist earth, from which he estimates the annual dew at 3. 28 inches. But it is probable that the dew which is deposited on grass is much more copious than what falls on moist earth, because grass exposes much more surface in a given acre of ground. If we take the dew at five inches annually, it will probably not be much over-rated: supposing it should be over-rated, the excess may stand against the rain that is lost by evaporation from the surface of the rain-gage each time it rains ‡. Wherefore, upon the whole, we shall

Estimate by Dr. Hales.

Annual quantity of dew taken at five inches.

* The editors of the Encyclopedia, under the article Weather, from 16 places of observation, make the annual mean for Great Britain 32. 53 inches; and M. Cotte, in the Journal de Physique for 1791, gives a mean derived from 147 places in different parts of the world equal to 34. 7 inches.

† Veg. Statics, Vol. I. page 52.

‡ Since writing the above paragraph on dew, I have had occasion to make several experiments on the subject of aqueous vapour, as it exists in the atmosphere, the result of which will, I am persuaded, materially illustrate this important question in physics.—At present I shall only observe, that the following conclusions seem deducible from the experiments above referred to.

Generalities concerning aqueous vapor.

1. That aqueous vapour is an elastic fluid *sui generis*, diffusible in the atmosphere, but forming no chemical combination with it.

2. That temperature alone limits the *maximum* of vapour in the atmosphere.

3. That

ſhall have 36 inches of water at a medium annually on the ſurface of the earth in England and Wales, reckoning 31 for rain and five for dew.

Annual quantity of rain and dew 36 inches.

According to Guthrie, the area of England and Wales is 46.450 ſquare miles. This reduced to ſquare feet, gives 1.378.586.880.000: which, multiplied by three feet the annual depth of rain and dew, gives 4.135.760.690.000 cubic feet of water = 153.176.320.000 cubic yards, or 28 cubic miles = 115 thouſand millions of tons in weight, nearly. We muſt now conſider how this enormous quantity of water is diſpoſed of.

Computed quantity of water that falls in a year 28 cubic miles, or 115 thouſand million of tons.

There are two principal ways by which the water derived from rain is carried off again: one part of it runs off immediately into rivulets, or ſinks into the earth a ſmall way, breaks out again in lower ground in the form of ſprings, thence makes its way to ſome river, by which it is conveyed into the ſea—another part is raiſed into the atmoſphere by evaporation. We take no notice here of the decompoſition of water by vegetables; becauſe it is preſumed that in the courſe of nature the principles are combined and water formed again.

It is carried off by rivers, by ſprings, and by evaporation.

3. That there exiſts at all times, and in all places, a quantity of aqueous vapour in the atmoſphere, variable according to circumſtances.

4. That whatever quantity of aqueous vapour may exiſt in the atmoſphere at any time, a certain temperature may be found, below which a portion of that vapour would unavoidably fall or be depoſited in the form of rain or dew, but above which no ſuch diminution could take place, chemical agency apart. This point may be called the *extreme temperature* of vapour of that denſity.

5. And that whenever any body colder than the extreme temperature of the exiſting vapour is ſituated in the atmoſphere, dew is depoſited upon it, the quantity of which varies as the ſurface of the body and the degree of cold below the extreme temperature.

N. B. The *extreme temperature* of vapour in the atmoſphere varies all the way from the *actual* temperature of the atmoſphere to 10, 15, 20 or more degrees below it.—The point may generally be found in the hotteſt months by pouring cold ſpring water into a dry and clean glaſs, and marking what degree of cold is ſufficient to produce a dew on the outſide of the glaſs; at other times frigorific ſaline ſolutions may be uſed.

SECTION 2.

An Estimate of the Quantity of Water that flows into the Sea from England and Wales in a Year.

Method of determining how much water flows through a river.

To calculate the quantity of water that flows down any one river into the sea in a given time, seems at first view a question of great difficulty. The necessary data, however, may be obtained with considerable exactness, by proper observations, and then it becomes an easy case of mensuration. Dr. Hutton, in his Philos. and Mathemat. Dictionary, article River, proposes a very good method to determine by experiment the velocity of a river :—A cylindrical piece of light wood, its length somewhat less than the depth of the waters, is to be taken, and a few small weights attached to one end in order to make it swim upright. To the other end a small rod is fixed in the centre in direction of the axis. This being suffered to float down the stream will move with the velocity of the water; and if the rod be observed to incline towards the river upward or downward, it shews the current to be more rapid at the bottom or surface respectively.

This experiment being made in the middle and near the sides of a river, a medium velocity may be obtained. Then the medium, breadth, depth, and space run over in a certain time being multiplied together, will give the quantity of water that flows down in that time.

Estimate of water delivered by the Thames.

Dr. Halley, in order to estimate the quantity of water that flows into the Mediterranean sea by means of rivers, makes a comparison of the great rivers of Italy, &c. with that of the Thames. (Philos. Transact. Abridg. Vol. 2. Page 110). He assumes the breadth of the Thames at Kingston Bridge to be 100 yards, its depth three yards, and velocity two miles per hour. He professedly over-rates the dimensions, in order to allow more than a sufficiency for the streams received below Kingston. This assumption gives the area of a transverse section of the river = 300 square yards, and the quantity of water flowing down = 20.300 000 tons in a day. This must be over-rated by at least, I think, one third :—If the breadth be assumed 100 yards, the depth three, and velocity two miles per hour, it will then give $\frac{2}{3}$ of the result above mentioned; or it will amount to the same thing if we take $\frac{1}{8}$ part from all the three data assumed by Dr. Halley, the result being $\frac{2}{3}$ of that above,

above, amounting in the year to 166.624.128.000 cubic feet, which is a little more than $\frac{1}{25}$ part of all the rain and dew in England and Wales in a year, as above deduced.

View of all the rivers of England and Wales,

By an inſpection of the annexed map of the rivers of this country, as well as by a fair calculation, it appears, that the water of the Thames is drawn from an extent of country of about 600 ſquare miles, or $\frac{1}{8}$ of the area of the whole, nearly. The Severn, including the Wye, ſpreads over an equal or greater extent of country: and that collection of rivers which conſtitutes the Humber is ſuperior to either of the other two in this reſpect. As far as my own obſervation goes, the Severn and Wye muſt diſembogue as much or more water than the Thames; the Humber I have not ſeen collectedly, but have noticed moſt of the branches conſtituting it, and ſhould apprehend it cannot be inferior to the Thames: all other circumſtances being the ſame, the quantity of water carried down by any river ſhould be as the area of the ground from which the water is derived, and on this account the Humber ought to exceed the Thames *.

and the diſtricts

The Severn, which is partly derived from the mountainous country of Wales, is certainly the moſt rapid of the three rivers, and probably carries down the moſt water: as the Thames, however, is generally conſidered to take the lead, we will ſuppoſe, upon the whole, that theſe three rivers are equal in this reſpect.

which ſupply them with water.

The counties of Kent, Suſſex, Hampſhire, Dorſetſhire, Devonſhire, Cornwall, and Somerſetſhire, from the Medway to the lower Avon incluſively, in an extent of 11.000 ſquare miles, do not preſent us with many large rivers. From their nnmber and magnitude, we cannot form a high eſtimate of their produce. The quantity of rain for thoſe counties is indeed near the average for the kingdom, as far as the preceding obſervations determine; but the milder temperature of their winters and greater heat of their ſprings and ſummers, will cauſe a greater evaporation than in ſome other parts: It is probable the rivers in theſe counties may amount, when taken together, to $1\frac{1}{2}$ times the magnitude of the Thames. The rivers that diſembogue their waters on the coaſt of Lin-

* A more perfect theorem will be given afterwards, for finding the quantity of water carried down by any river.

colnſhire,

colnſhire, Norfolk, Suffolk and Eſſex, from the Humber to the Thames, though drawn from a country of 7000 ſquare miles, manifeſtly fall far ſhort of the Thames. The two places in this diſtrict, for which we have accounts of the rain, Norwich and Upminſter. give a mean of only 22½ inches annually. This, with the flatneſs of the country, which prevents the water from running off in ſome degree, makes the rivers much leſs than what might otherwiſe be expected from the extent of ground. There are but three or four of any conſequence, Probably all the rivers may amount to half the ſize of the Thames. There remains above 6000 ſquare miles in Wales, from the Wye to the Dee, incluſive of the laſt, and the northern counties of Lancaſter, Weſtmoreland, Cumberland, Northumberland, and Durham, with part of Cheſhire and a ſmall part of Yorkſhire, from the Merſey round by the Tweed to the Tees, amounting to 7 or 8000 ſquare miles, to be eſtimated.

The rivers upon the whole are conſidered as carrying off 13 inches out of the 36 in their fall annually.

Theſe two diviſions, though not larger than ſome others, abound in rivers, many of which are conſiderable in magnitude and of great rapidity. The rains at an average, it is probable, are double what they are in the S. E. counties of the kingdom. The rivers in theſe two diſtricts cannot fairly be eſtimated, I think, at leſs than *four* times the Thames. It appears, then, that by this eſtimation, the water carried off by all the rivers in England and Wales, may amount to *nine* times that carried off by the Thames = 13 inches of rain. There remains ſtill *ſixteen* times the water of the Thames, or 23 inches of rain to account for, before we have diſpoſed of all the rain and dew.

SECTION 3.

An Eſtimate of the Quantity of Water raiſed by Evaporation.

Evaporation

Upon looking over the ſurface of any country, three principal varieties of ſurface preſent themſelves to view, as far as reſpects evaporation, namely, *water*, ground covered with graſs and other vegetables, and bare ſoil. The difficulties that occur in attempts to find the quantity of water evaporated in thoſe three caſes, are perhaps the principal reaſon why our knowledge on this head is ſo imperfect.

from water, bare ſoil, and covered ground.

As far as experiments hitherto made authoriſe us to draw concluſions, it ſhould ſeem that the evaporation from water is greateſt;

greateſt; that from green ground is probably next, and that from bare ſoil the leaſt: though we may preſume, that the copious dews upon the graſs more than ſupply the exceſs of evaporation above what takes place from a moiſt uncovered ſoil.

The moſt ſatisfactory experiments I have ſeen an account of, relating to the evaporation from a ſurface of water, are thoſe of Dr. Dobſon, made at Liverpool, in the years 1772, 73, 74 and 75. (Vid. Philos. Tranſac. Vol. 67.)—He took a cylindrical veſſel of 12 inches diameter, and having nearly filled it with water, expoſed it beſides his rain-gage of the ſame aperture, and by adding water to it, or taking it away occaſionally, he kept the ſurface nearly of the ſame height, and carefully regiſtered the quantities added or taken away. by a compariſon of which with the rain, the amount of the evaporation was aſcertained. The mean monthly evaporation for four years was—January 1.50 inches.—February 1.77. —March 2.64.—April 3.30.—May 4.34.—June 4.41.—July 5.11.—Auguſt 5.01.—September 3.18.—October 2.51.—November 1.51.—December 1.49.—In all 36.78 inches. The mean rain for the ſame time was 37.48 inches.—In the year 1793 I found the evaporation from water in a ſimilar way at Kendal for 82 days in March, April, May and June to be 5.414 inches. The greateſt quantity evaporated on one of the hotteſt and drieſt days in ſummer was a little above ,2 of an inch in depth.

Dr. Dobſon's experiments on evaporation from water.

The experiments to determine how much is evaporated from green ground and from moiſt earth, are very few that have come to my knowledge. Dr. Hales, from a few experiments, calculates that moiſt earth only throws off $6\frac{2}{3}$ inches annually.—This calculation muſt be far below the truth. Dr. Watſon, Biſhop of Llandaff, found that in a dry ſeaſon there evaporated from a graſs plat that had been mowed cloſe, about 1600 gallons in an acre per day, which amounts nearly to ,07 of an inch in depth; and that after rain the evaporation was conſiderably more. Now ſuppoſing ,07 to be the medium daily evaporation for May, June, July and Auguſt, and that as much is raiſed in theſe four months as in all the reſt of the year, the annual evaporation in ſuch circumſtances will be 17 or 18 inches, which is but half that obſerved from water at Liverpool, and ſix inches leſs than the reſerve of rain ſtated above.

Hales's eſtimate from moiſt earth.

Dr. Watſon from a graſs plat.

In

Experiment on a tube of earth to ſhew how much water ran off the ground; how much ſunk in; and how much evaporated.

In order to aſcertain this point more fully, and to inveſtigate the origin of ſprings, my friend Thomas Hoyle, jun. and ſelf, practiſed an expedient as follows, beginning in the autumn of 1795. Having got a cylindrical veſſel of tinned iron, 10 inches in diameter and three feet deep, there were inſerted into it two pipes turned downwards for the water to run off into bottles: the one pipe was near the bottom of the veſſel; the other was an inch from the top. The veſſel was filled up for a few inches with gravel and ſand, and all the reſt with good freſh ſoil. It was then put into a hole in the ground, and the ſpace around filled up with earth, except on one ſide, for the convenience of putting bottles to the two pipes; then ſome water was poured on to ſadden the earth, and as much of it as would was ſuffered to run through without notice, by which the earth might be conſidered as ſaturated with water. For ſome weeks the ſoil was kept above the level of the upper pipe, but latterly it was conſtantly a little below it, which precluded any water running off through it. Moreover, for the firſt year the ſoil at top was bare; but for the two laſt years it was covered with graſs the ſame as any green field. Things being thus circumſtanced, a regular regiſter has been kept of the quantity of rain water that ran off from the ſurface of the earth through the upper pipe (whilſt that took place) and alſo of the quantity of that which ſunk down through the three feet of earth, and ran out through the lower pipe. A rain-gage of the ſame diameter was kept cloſe by to find the quantity of rain for any correſponding time.

The

The following Table shews the Result. Tabulated results.

	Water through the two Pipes.			Mean.	Mean Rain.	Mean Evap.
	Inch. 1796.	Inch. 1797.	Inch. 1798.	inch.	inch.	inch.
Jan.	1.897—	,680—	1.774+	1.450+	2.458	1.008
Feb.	1.778—	,918—	1.122	1.273	1.801	,528
March	,431—	,070—	335	,279	,902	,623
April	,220—	,295—	,180	,232	1.717	1.485
May	2.027—	2,443+	,010	1.493+	4.177	2.684
June	,171—	,726—	——	,299	2.483	2.184
July	,153—	,025	——	,059	4.154	4,095
Aug.	——	——	,504	,168	3.554	3.386
Sept.	——	,976	——	,325	3.279	2.954
Oct.	——	,680	——	,227	2.899	2.672
Nov.	——	1.044	1.594	,879	2.934	2.055
Dec.	,200	3.077	1,878+	1.718+	3.202	1.484
	6.877—	10.934—	7,379	8.402	33.560	25.158
Rain	30.629—	38.791—	31,259			
Evap.	23.725—	27.857—	23.862			

The following observations were made when the water passed through both pipes: that is, when the vessel was filled up with earth above the level of the upper pipe. Variation of the experiments.

		Top pipe Inch.	Bottom pipe. Inch.
1796. Jan.	25	,190	,280
	30	,080	,114
Feb.	2	,100	,254
	8	,196	,140
May	1	,163	,000
	10	,060	,400
	12	,312	,175
	15	,190	,200
June	3	,120	,040
	Total	1.411	1.603

The column of mean evaporation is derived by taking the difference of the two columns preceding it; but it should be observed that though this method is sufficiently exact in taking the Remarks on the tables.

the year together, it is not ſo in taking the months ſeverally, becauſe it preſumes that the earth in the veſſel contains the ſame quantity of water at the end of each month, or is ſaturated with it; whereas in the ſummer months it is frequently ſhort of ſaturation. The conſequence is, that the evaporation appears from this table to be ſomething leſs than it really is in the ſummer months, and ſomething more in the antumnal *.

Concluſions. Evapor. from ground is 30 inches.

From theſe experiments it ſeems we may conclude—1ſt. That the quantity of water evaporated, *in the circumſtances above related,* amounts to 25 inches of rain annually; to which if we add five inches for the dew, it will give 30 inches of water raiſed annually.

It increaſes with the rain.

2d. That the quantity of evaporation increaſes with the rain, but not proportionally. Thus, 1797 gave the moſt rain and the greateſt evaporation, &c.

Deep bare ſoil and ſod do not differ.

3d. That it does not appear there is much difference betwixt the evaporation from bare earth, when there is ſufficient depth of ſoil, and that from ground covered with vegetating graſs. The account in 1796 is much what might have been expected, if the earth had been covered with graſs.

Whether this evap. of 30 and the rivers of 13, making 43 inſtead of 36 (the rain) indicate another ſupply?

As this account of evaporation, amounting to 30 inches, exceeds the medium reſerve of rain of 23 inches, it demands an enquiry whether the rain is adequate, or whether the earth derives a ſupply of water from ſome ſubterranean reſervoir, according to the opinion of ſome philoſophers.

Reply; negative.

With reſpect to the deficiency of 7 inches, there are *three* cauſes to be aſſigned for it, which appear to me fully ſufficient, without having recourſe to any ſource but that of rain for the ſupply of the earth in general.

The evapor. was taken a little too great.

1ſt. In the account of the rain that paſſed through the earth in our evaporating veſſel, there are a few monthly products marked, + thoſe were occaſioned by the bottle that received the water through the pipe being found with the water running over; this loſs was placed to the account of evaporation; it could not be much, as the water was taken ſeveral times in a month, but poſſibly might amount to one inch in the year.

The rain at Mancheſter exceeds the medium,

2d. The rain at Mancheſter, being $33\frac{1}{2}$ inches annually, exceeds the medium of 31 inches; and conſequently, ac-

* N. B. The earth in the veſſel always appeared as well ſupplied with moiſture as the ground around it, in the drieſt weather.

cording

cording to the preceding obfervations, the evaporation ought to exceed the medium.

3d. But the principal caufe of the excefs in our account of evaporation, I conceive to be the prevention of the water running off from the furface of the earth at the top, by having the earth below the level of the upper pipe: it has been feen, that when the earth was above that level, a great part of the water came off that way, by which the furface was fooner dried: whereas by forcing all the water to fink through the earth or ftand on its furface, a greater degree of moifture perpetually exifted at the furface, and confequently afforded a greater fcope for evaporation, than the furface of the earth in general would do. and the water not being fuffered to run freely off at top, did increafe the evaporation.

Upon the whole then I think we may fairly conclude—that the *rain* and *dew* of this country are equivalent to the quantity of water carried off by evaporation and by the rivers. And as nature acts upon general laws, we ought to infer, that it muft be the cafe in every other country, till the contrary is proved. Hence the rain and dew fully fupply the evaporation and the rivers.

This conclufion being admitted, we are enabled to deduce a general theorem for the quantity of water carried down into the fea by any river in any country (on the fuppofition that all rivers are ramified alike) provided we have certain *data*: thefe *data* are the length of the river, and the excefs of the rain above the evaporation in the country from which the water of the river is drawn: alfo, it fhould be known by obfervation, how much water fome one given river carries down. Theorem for deducing the quantity of water carried by a river, &c.

For, from the principles of geometry, the area of country from which any river is fupplied, will be as the fquare of the length of the river; and the quantity of water carried off, will be in the compound ratio of the area of the country, and the excefs of the rain and dew above the evaporation.

Thus, let L = the length of any river, E = the excefs of rain and dew above the evaporation, and Q = the quantity of water difembogued in any given time by that river; l = the length of any other river, e = the excefs, &c. and q = the quantity of water; then we fhall have $q = \frac{Q\,l\,e}{L\,E}$. Statement.

Ex. gr. Suppofe the length of the Thames = 200 miles, and the excefs = 5 inches, eftimating the rain and dew at 30 inches and evaporation at 25; and fuppofe the river Kent, in Weftmorland, Example.

Weſtmorland, to be 20 miles in length, and the exceſs 35 inches, the rain and dew being ſuppoſed 65, and evaporation 30 inches.

Then, $\frac{20^2 \times 35 \times Q}{200^2 \times 5} = \frac{7\,Q}{100} = q$ or $Q = 14\frac{2}{7}q$; which reſult, I believe, will be found to accord nearly with the meaſurement of the two rivers on the principle before mentioned.

SECTION 4.

On the Origin of Springs.

Cauſe of ſprings.

The origin of ſprings has always been juſtly conſidered as a queſtion of natural hiſtory worthy of inveſtigation.—In the infancy of ſcience hypotheſes are formed to account for phenomena; but when facts are diſcovered totally inconſiſtent with an hypotheſis, it ought to be diſcarded. This does not ſeem to have been the caſe in the ſubject before us; for various opinions are ſtill held by ſome, which it is impoſſible to ſupport by facts. The object of the following remarks and experiments is to aſcertain the diſputed point if poſſible.

Opinions.

There are *three* opinions reſpecting the origin of ſprings which it may be proper to notice.

1. Rain and dew.

1ſt. That they are ſupplied entirely by rain and dew.

2. Subterranean reſervoirs.

2d. That they are principally ſupplied by large ſubterranean reſervoirs of wates.

3. Filtration from the ſea.

3d. That they derive their water originally from the ſea, on the principle of filtration.

Whether the firſt cauſe be inſufficient.

It is obvious, that before we pay any attention to the two latter opinions, the cauſes aſſigned in the firſt ought to be proved inſufficient by direct experiment. M. de la Hire is the only one who has attempted to do this, as far as my information extends, in the Pariſian Memoirs for 1703. He procured a leaden veſſel eight feet deep, having a pipe at the bottom; this he buried in the earth, and filled with ſoil of ſand and loam, expoſing the ſurface to receive all the rain that fell. After 15 years trial, he found that no water had run through the pipe at the bottom.

De la Hire's experiment. No rain ſoaked through a maſs of earth eight feet thick.

Shallower veſſel.

Again, he took another veſſel, eight inches deep, which he filled with earth and expoſed in like manner. No rain penetrated ſo as to run out at the bottom from June to February;

 but

but after that time it yielded a quantity after moſt rains. Another veſſel of twice the depth, or 16 inches, gave a reſult much like that of eight inches. Farther, M. de la Hire found, that when herbs were planted in the ſoil of the laſt mentioned veſſel, and grown up, no rain penetrated through the ſoil, but inſtead thereof it was not ſufficient to ſuſtain the vegetation; for the plants would require to be ſprinkled occaſionally, or elſe they began to droop and wither.

Remarks. Paris has only 20 inches annual rain;

With reſpect to the firſt mentioned fact, we need not wonder that no water penetrated through eight feet of earth at Paris, where the annuul rain is but 20 inches, when only eight or nine inches penetrated through three feet of earth here, where the rain is 33 or 34 inches annually. But it does not follow that rain may not deſcend down declivities of the ground into vallies or lower parts, at Paris as well as here, and being accumulated may penetrate into the earth to a conſiderable depth, eſpecially if it meet with channels or chaſms of any kind, or declining ſtrata of earth that are impenetrable by water. Paris, I believe, however, is not very liberally ſupplied with ſprings, as might be expected. As to the experiment upon vegetation, it only proves that the rain in ſpring and ſummer is ſometimes not ſufficient to ſupport vegetable life, a fact which may readily be granted; but then in his experiment the plants were precluded from a ſupply of moiſture from the earth beneath the veſſel, which is a reſerve of the utmoſt conſequence in dry ſeaſons.

but the higher grounds there as well as here may afford waters, &c.

Water after ſoaking into the earth aſcends again.

This circumſtance of water aſcending again in the earth, on whatever principle it is effected, cannot be denied.—There were $4\frac{3}{4}$ inches of rain here in July laſt, none of which paſſed through the earth in the evaporating veſſel; this earth, however, at the end of the month, was far from that degree of dryneſs which is unfit for the ſupport of vegetation.—During the firſt four days of Auguſt there fell about three inches of rain, and only $\frac{1}{2}$ an inch penetrated through the earth in the evaporating veſſel. Conſequently three feet in depth of earth that was moderately moiſt imbibed nearly three inches of rain before it was ſaturated; whence we may conclude that three inches nearly had aſcended and been evaporated. This evidently ſhews, that earth is capable of holding a very great proportion of water, that in ſummer the water aſcends to ſupply

ply the exigencies at the ſurface, and that earth far under the point of ſaturation with moiſture is ſtill fit to ſupport vegetation.

Queſtion. How much water is contained in ſaturated ground.

This obſervation ſuggeſted the following queſtion—How much water is there in a given depth of earth when the ſoil is at the point of ſaturation, or in that ſtate when it begins to yield water from the lower pipe of the evaporating gage?

Experiment. 1 foot ſaturated earth contains 7 inches water, and it may loſe half before it is too dry for vegetation.

To determine this I took a quantity of garden ſoil that had been ſoaked with rain a day before, and preſſed it into a crucible; in this ſtate I found its ſpecific gravity to that of water as five to three. It was then expoſed to a moderate heat till it appeared, as near as I could judge, of the ſame moiſture as garden ſoil two inches deep in dry ſummer weather; afterwards it was expoſed almoſt to a red heat till it became a perfectly dry powder; in the former caſe it loſt $\frac{1}{12}$ of its weight, and in the latter $\frac{1}{3}$.—When it had loſt $\frac{1}{6}$, it did not appear too dry to ſupport vegetation. When it had loſt $\frac{2}{9}$, it appeared like the top ſoil in ſummer.—Hence it follows, that every foot of earth in depth, ſo ſaturated, contains ſeven inches of water, and that it may part with one quarter of its water, or even one half, and not be too dry for ſupporting vegetation.

Brick clay nearly the ſame.

Clay, juſt dug out for the purpoſe of making bricks, was tried in the ſame manner: It gave the ſame ſpecific gravity as the earth, and yielded not much leſs water.

De la Hire's concluſions erroneous.

Theſe experiments and obſervations prove, that M. de la Hire's concluſions, drawn from the vegetation of plants in a given quantity of ſoil, precluded from any communication with the earth at large, are erroneous, or at leaſt unwarranted: As it does not thence appear that the evaporation for the *whole* year exceeds the rain in the year, whatever it may do for a month or two in ſummer.

Springs are therefore ſupplied by the rains.

The origin of ſprings may ſtill therefore be attributed to rain, till ſome more deciſive experiments appear to the contrary; and it becomes unneceſſary to controvert the other two opinions reſpecting this ſubject.

Springs are thence deficient in ſummer.

Upon the whole it ſhould ſeem, that at the commencement of ſpring, the ground is nearly ſaturated with water for five or ſix feet in depth, as the rains and dews in autumn and winter far exceed the evaporation: There are then five or ſix inches of water at leaſt to be raiſed up again to the ſurface in caſe

cafe of exigence in the fpring and fummer: If this happen to be fo, then it is at the expence of fprings; for we find the generality of fprings become languid, or entirely ceafe to flow at the end of a long drought. As to the few fprings that feem to be little affected by dry or wet feafons, they form exceptions which it would not be difficult to account for.

VII.

Defcription of an Inftrument for extracting Hard Subftances which may ftick during their Paffage to the Stomach. By G. C.

To Mr. NICHOLSON.

SIR,

ENCLOSED, I fend you a drawing of an untried inftrument; if you think it likely to accomplifh the end for which it was defigned, you will, perhaps, give it a place in your very ufeful work. This inftrument, I conceive to be an improvement of that commonly ufed for forcing down any hard fubftances that may ftick between the mouth and the ftomach. In many cafes nails, pins, and other metallic matters, get into this fituation, when it would undoubtedly be preferable to draw them up through the mouth, inftead of paffing them into the ftomach, where they are no fooner arrived, than they furnifh a new fpecies of danger to the fufferer. With this view the following inftrument was conftructed. A, B, Pl. XI. is a rod of whalebone, having a fmall groove down the middle, from end to end, large enough to contain a ftrong filken thread; this thread is confined to the groove by a few lappings of fine waxed filk. At B, is faftened, as ufual, a fponge, about one third of the common fize. Juft above the fponge is fixed a fmall pulley, round which the filken thread winds, and returning up the oppofite fide of the whalebone to that on which it defcended, is tied faft to the bottom of a fmall leather cap C. Above this cap, at D, are faftened 12 or 14 fmall filver wires, made to fpring into the form reprefented in Fig. 2. Thefe wires by means of loops at their ends, fupport a round bag of net-work of fine filk, perforated in the centre to admit the whalebone rod. Thefe wires, together with the bag, muft be capable of being inferted and confined in the cap C, fo

Inftrument for extracting bodies from the throat.

Defcription.

ſo as to remain in the poſition repreſented in Fig. 1. in which ſtate it is ready for being paſſed down the oeſophagus; and it ſeems ſcarcely neceſſary to remark, that previous to the inſtrument being withdrawn, the cap C muſt be pulled off, by means of the thread at A; when the wires will expand the net, and preſs its edge cloſe to the gullet on every ſide, and in returning to the mouth, will probably bring up any ſubſtance that was lodged there, within it.

G. C.

Brompton, Jan. 23, 1803.

VIII.

On the Flexure of Wax and other Bodies by irregular cooling, with Conſiderations on the Probability that it may be cauſed by the Law of Cryſtallization. In a Letter from R. B.

To Mr. NICHOLSON.

London, Feb. 16, 1803.

SIR,

Thread of wax flowing down a candle,

A STREAM of wax has juſt overflowed the cup of the wax candle by which I have been reading, and has preſented me with a fact or two which I think worthy the meditation of philoſophers. If you think ſo, pleaſe to give them a place in your repoſitory.

becomes ſeparated by ſpontaneous flexure.

The fluid wax has formed a line or protuberance on the outſide of the candle, four inches in length, a little more than one fifteenth of an inch in width, or ſurface applied to the candle, and one tenth of an inch in elevation or thickneſs. As it grew cold it has ſeparated from the candle, ſo that its lower extremity ſtands a little more than half an inch diſtant, and it does not touch for the length of two inches and a half from the bottom; all the upper part ſtill continuing adherent. And laſtly, the ſeparated portion exhibits a regular curve, convex towards the candle, and more convex the nearer the lower extremity; ſo that, when carefully taken off, and applied to ſeveral circular arcs deſcribed upon paper, the difference was very obviouſly perceived.

The wax was cooled by different parts in ſucceſſion.

Upon theſe facts I obſerve from obvious reaſons that the wax which flowed in contact with the ſolid candle, was more ſpeedily

ſpeedily cooled than that which flowed on the outſide of that heated wax; and that that portion of the wax which flowed in contact with the candle, and ran to the greateſt diſtance was the moſt ſpeedily cooled.

And the reſult farther ſhews, 1. That the dimenſions of wax ſuddenly cooled, are larger than thoſe of wax cooled more ſlowly; and 2. The quicker the cooling the greater this difference. For the fluid wax was depoſited in a ſtrait line, and its curve figure after cooling ſhewed that the interior line or convex limit was longer then the exterior or concave limit; and this difference being greateſt where the refrigeration was moſt ſudden, namely, towards the lower extremity, was ſhewn in the greater curvature. Deduction. Wax quickly cooled is leſs denſe than if ſlowly cooled.

I will not, Sir, do your readers the injuſtice to ſuppoſe any of them will think the dignity of philoſophy impaired by a ſpeculation on the guttering of a candle, but will proceed in my diſquiſition in hopes that others of more leiſure and ability may purſue the object farther, if found to deſerve it. And as it has been a faſhion ſince the publication of the famous ſtring of queries at the end of Sir Iſaac Newton's Optics, for ſpeculators to uſe that modeſt term to dignify inductions which they may ſuppoſe to be almoſt proved, I will take the liberty to offer a few on the preſent occaſion. Apologetical remark.

1. Since the ſpecific gravity of ſteel ſuddenly cooled, is leſs than when annealed or ſlowly reduced in temperature; ſince ice and other cryſtals ſlowly formed, are generally underſtood to be denſer than the products of haſty refrigeration; and ſince in our experiment wax obeys the ſame law, is it not probable that the law may be general in the cooling of all bodies. *Fiant Experimenta.* Qu. 1. Are not all bodies rarer if more ſpeedily cooled, congealed or cryſtallized?

2. Can this effect be aſcribed to any thing but the arrangement of the parts of bodies; and if ſo, is it not referable to haſty and ſlow cryſtallization; and are we not therefore juſtifiable in ſuppoſing that the cryſtallization of bodies may be altered, even in the ſolid ſtate, as in the hardening or ſoftening of ſteel. Qu. 2. Is not this cryſtallization? even in bodies already ſolid?

3. As the hardneſs of ſteel becomes greater and its tenacity leſs by ſudden refrigeration, is it not probable that all the products of haſty cryſtallization are harder as they are known to be more brittle. Try in ſal ammoniac whether its ſoftneſs or flexibility will become leſs by ſudden cooling, ſuch as ſubli- Qu. 3. Are not all theſe bodies more hard and leſs tenacious?

mation in an head covered with ice; and examine whether an unannealed glafs tube be not harder than a portion of the fame tube flowly cooled.

Qu. 4. Will it not be ufeful to repeat this experiment with other bodies?

4. May not fome ufeful indications as to the properties of metals and other bodies be derived from the fimple procefs of cafting a long flip of them, by pouring the fufed metal upon a cold ftone. If the curvature be univerfally like that of the wax, the law will be more and more confirmed. If anomalies prefent themfelves, our acquaintance with natural events will neverthelefs be extended and improved.

Experiments.

Since writing the above, I have taken fome of the materials next at hand to make an experiment or two, which I give without regarding whether they fupport, modify, or deftroy the hypothefis advanced above.

Exp. 1. Annealed and unannealed glafs, The annealed feems hardeft.

Exp. 1. A thick glafs tube which had been bended into a fyphon, was broken in two at the place of flexure. It was fuppofed that the bended part having been heated and cooled a fecond time, might prove fofter from this kind of annealing. The extremity of the ftrait part was applied to fcratch the bended part, and alfo the other ftrait part; and contrarywife the extremity of one of the bended parts was applied to the other portion of tube; and laftly both ends were fcratched with a file. No certainty was obtained, but it was thought that the bended part was hardeft *.

Exp. 2. Lead caft on a flab. No refult.

Exp. 2. Lead was fufed and poured red hot upon a marble flab in a long flip. Other lead moderately heated was poured out. The pieces did not quit the face of the ftone in cooling, and they were too flexible to be taken up and examined.

Exp. 3. Type metal. Bended like the wax.

Exp. 3. Type metal was treated in the fame manner. A flat piece one thirtieth of an inch thick, and fix inches long, bended upwards from the ftone in cooling, to the height or verfed fine of one twentieth of an inch, and retained its flexure.

Exp. 4. Fufible metal. Bended very much when nearly cold, *contrary to the wax, and recovered itfelf afterwards.*

Exp. 4. Fufible metal or the compound of lead, tin, and bifmuth, was poured out in a flip 13 inches long, one half being about the fame thicknefs or lefs than the type metal,

* As the tube had been bended with the blow pipe, the difference might as well have arifen from oxigenation of the lead or manganefe, or diffipation of alkali, as from fimple heating and cooling.

R. B.

and

and the other half confiderably thicker. It did not appear to rife from the flab; but was taken up while quite warm and held with its edge downwards, to prevent any flexure from its weight. The thin part immediately began to bend, and the thick part foon afterwards, *the flexures being concave on the fide which had touched the ftone,* and the verfed fine or height of the curvature was one inch at the end of half a minute, after which it gradually ftraightened itfelf as follows,

Bar of Fufible Metal 13 *Inches long.*

At 8^h	51^m	taken up ftrait.		inch.
	$51\frac{1}{2}$	it became bended;	verfed fine	1.00
	54	- - lefs bended	- do. -	0.7
	55	- - ————	————	0.4
	57	- - ————	————	0.25
	60	thin part ftrait; thick a little curved.		

Another experiment had been made before, without any expectation of fuch or fo great a flexure, and no meafures were then taken. In this the bar was thinner and the flexure greater; and the return to ftraitnefs was not only complete, but one end which was very thin became at laft bended the contrary way, namely the face that had touched the ftone was convex.

A fmaller piece of two inches in length which was not taken up fo foon, had become bended as it lay; fo that it refted on its two extremities and was hollow in the middle. This recovered its firft ftraitnefs as it lay on the table.

I am

SIR,

your obliged reader,

R. B.

IX. *A*

IX.

An Account of ſome Caſes of the Production of Colours, not hitherto deſcribed. By THOMAS YOUNG, *M. D. F. R. S. F. L. S. Profeſſor of Natural Philoſophy, in the Royal Inſtitution* *.

Simple and general law reſpecting two portions of the ſame light arriving by different routes;

WHATEVER opinion may be entertained of the theory of light and colours which I have lately had the honour of ſubmitting to the Royal Society, it muſt at any rate be allowed that it has given birth to the diſcovery of a ſimple and general law, capable of explaining a number of the phenomena of coloured light, which, without this law, would remain inſulated and unintelligible. The law is, that "wherever two portions of "the ſame light arrive at the eye by different routes, either "exactly or very nearly in the ſame direction, the light be-"comes moſt intenſe when the difference of the routes is any "multiple of a certain length, and leaſt intenſe in the inter-"mediate ſtate of the interfering portions; and this length is "different for light of different colours."

which explains many phenomena.

I have already ſhown in detail, the ſufficiency of this law for explaining all the phenomena deſcribed in the ſecond and third books of Newton's Optics, as well as ſome others not mentioned by Newton. But it is ſtill more ſatisfactory to obſerve its conformity to other facts, which conſtitute new and diſtinct claſſes of phenomena, and which could ſcarcely have agreed ſo well with any anterior law, if that law had been erroneous or imaginary: theſe are, the colours of fibres, and the colours of mixed plates.

and new facts.

Production of colours by a minute fibre near the edge of an obſtacle intercepting light &c.

As I was obſerving the appearance of the fine parallel lines of light which are ſeen upon the margin of an object held near the eye, ſo as to intercept the greater part of the light of a diſtant luminous object, and which are produced by the fringes cauſed by the inflection of light already known, I obſerved that they were ſometimes accompanied by coloured fringes, much broader and more diſtinct; and I ſoon found, that theſe broader fringes were occaſioned by the accidental interpoſition of a hair. In order to make them more diſtinct, I employed a horſe-hair; but they were then no longer viſible. With a

* Philos. Trans. 1802.

fibre of wool, on the contrary, they became very large and confpicuous: and, with a fingle filk-worm's thread, their magnitude was fo much increafed, that two or three of them feemed to occupy the whole field of view. They appeared to extend on each fide of the candle, in the fame order as the colours of thin plates, feen by tranfmitted light. It occurred to me, that their caufe muft be fought in the interference of two portions of light, one reflected upon the fibre, the other bending round its oppofite fide, and at laft coinciding nearly in direction with the former portion; that accordingly as both portions deviated more from a rectilinear direction, the difference of the length of their paths would become gradually greater and greater, and would confequently produce the appearances of colour ufual in fuch cafes; that, fuppofing them to be inflected at right angles, the difference would amount nearly to the diameter of the fibre, and that this difference muft confequently be fmaller as the fibre became fmaller; and, the number of fringes in a right angle becoming fmaller, that their angular diftances would confequently become greater, and the whole appearance would be dilated. It was eafy to calculate, that for the light leaft inflected the difference of the paths would be to the diameter of the fibre, very nearly as the deviation of the ray, at any point, from the rectilinear direction, to its diftance from the fibre.

Remarks and inferences. A portion of light reflected from one fide nearly coincides in direction with another portion inflected; but their lengths differ and produce colour.

I therefore made a rectangular hole in a card, and bent its ends fo as to fupport a hair parallel to the fides of the hole; then, upon applying the eye near the hole, the hair of courfe appeared dilated by indiftinct vifion into a furface, of which the breadth was determined by the diftance of the hair and the magnitude of the hole, independently of the temporary aperture of the pupil. When the hair approached fo near to the direction of the margin of a candle that the inflected light was fufficiently copious to produce a fenfible effect, the fringes began to appear; and it was eafy to eftimate the proportion of their breadth to the apparent breadth of the hair, acrofs the image of which they extended. I found that fix of the brighteft red fringes, nearly at equal diftances, occupied the whole of that image. The breadth of the aperture was $\frac{66}{1000}$, and its diftance from the hair $\frac{8}{10}$ of an inch: the diameter of the hair was lefs than $\frac{1}{500}$ of an inch; as nearly as I could afcertain, it was $\frac{1}{600}$. Hence we have $\frac{11}{1000}$ for the

Precife repetition of the experiment

the deviation of the firſt red fringe at the diſtance $\frac{8}{10}$; and, as $\frac{8}{10} : \frac{11}{1000} :: \frac{1}{600} : \frac{11}{480000}$, or $\frac{1}{43636}$ for the difference of the routes of the red light where it was moſt intenſe. The meaſure deduced from Newton's experiments is $\frac{1}{39200}$. I thought this coincidence, with only an error of one-ninth of ſo minute a quantity, ſufficiently perfect to warrant completely the explanation of the phenomenon, and even to render a repetition of the experiment unneceſſary; for there are ſeveral circumſtances which make it difficult to calculate much more preciſely what ought to be the reſult of the meaſurement.

The halos round a diſtant candle ſeen through wool.

When a number of fibres of the ſame kind, for inſtance, a uniform lock of wool, are held near to the eye, we ſee an appearance of halos ſurrounding a diſtant candle: but their brilliancy, and even their exiſtence, depends on the uniformity of the dimenſions of the fibres; and they are larger as the fibres are ſmaller. It is obvious that they are the immediate conſequences of the coincidence of a number of fringes of the ſame ſize, which, as the fibres are arranged in all imaginable directions muſt neceſſarily ſurround the luminous object at equal diſtances on all ſides, and conſtitute circular fringes.

Coloured atmoſpheric halos.

There can be little doubt that the coloured atmoſpherical halos are of the ſame kind: their appearance muſt depend on the exiſtence of a number of particles of water, of equal dimenſions, and in a proper poſition, with reſpect to the luminary and to the eye. As there is no natural limit to the magnitude of the ſpherules of water, we may expect theſe halos to vary without limit in their diameters; and, accordingly, Mr. Jordan has obſerved that their dimenſions are exceedingly various, and has remarked that they frequently change during the time of obſervation.

New colours ſeen through two plates of glaſs with a little moiſture between them mixed with air.

I firſt noticed the colours of mixed plates, in looking at a candle through two pieces of plate-glaſs, with a little moiſture between them. I obſerved an appearance of fringes reſembling the common colours of thin plates; and, upon looking for the fringes by reflection, I found that theſe new fringes were always in the ſame direction as the other fringes, but many times larger. By examining the glaſſes with a magnifier, I perceived that wherever theſe fringes were viſible, the moiſture was intermixed with portions of air, producing an appearance ſimilar to dew. I then ſuppoſed that the origin of the colours was the ſame as that of the colours of halos; but, on

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a more minute examination, I found that the magnitude of the portions of air and water was by no means uniform, and that the explanation was therefore inadmiſſible. It was, however, eaſy to find two portions of light ſufficient for the production of theſe fringes; for, the light tranſmitted through the water, moving in it with a velocity different from that of the light paſſing through the interſtices filled only with air, the two portions would interfere with each other, and produce effects of colour according to the general law. The ratio of the velocities in water and in air, is that of 3 to 4; the fringes ought therefore to appear where the thickneſs is 6 times as great as that which correſponds to the ſame colour in the common caſe of thin plates; and, upon making the experiment with a plane glaſs and a lens ſlightly convex, I found the ſixth dark circle actually of the ſame diameter as the firſt in the new fringes. The colours are alſo very eaſily produced, when butter or tallow is ſubſtituted for water; and the rings then become ſmaller, on account of the greater refractive denſity of the oils: but, when water is added, ſo as to fill up the interſtices of the oil, the rings are very much enlarged; for here the difference only of the velocities in water and in oil is to be conſidered, and this is much ſmaller than the difference between air and water. All theſe circumſtances are ſufficient to ſatisfy us with reſpect to the truth of the explanation; and it is ſtill more confirmed by the effect of inclining the plates to the direction of the light; for then, inſtead of dilating, like the colours of thin plates, theſe rings contract: and this is the obvious conſequence of an increaſe of the length of the paths of the light, which now traverſes both mediums obliquely; and the effect is every where the ſame as that of a thicker plate.

Theſe were not produced by inflection and reflection but by the different velocities of the light through water and air.

Proofs of this theory.

It muſt however be obſerved, that the colours are not produced in the whole light that is tranſmitted through the mediums: a ſmall portion only of each pencil, paſſing through the water contiguous to the edges of the particle, is ſufficiently coincident with the light tranſmitted by the neighbouring portions of air, to produce the neceſſary interference; and it is eaſy to ſhow that, on account of the natural concavity of the ſurface of each portion of the fluid adhering to the two pieces of glaſs, a conſiderable portion of the light which is beginning to paſs through the water will be diſſipated laterally by reflection

The curved figure of the particles cauſe much of the light to deviate and modify the effects.

tion at its entrance, and that much of the light paſſing through the air will be ſcattered by refraction at the ſecond ſurface. For theſe reaſons, the fringes are ſeen when the plates are not directly interpoſed between the eye and the luminous object; and, on account of the abſence of foreign light, even more diſtinctly than when they are in the ſame right line with that object. And, if we remove the plates to a conſiderable diſtance out of this line, the rings are ſtill viſible, and become larger than before; for here the actual route of the light paſſing through the air, is longer than that of the light paſſing more obliquely through the water, and the difference in the times of paſſage is leſſened. It is however impoſſible to be quite confident with reſpect to the cauſes of theſe minute variations, without ſome means of aſcertaining accurately the forms of the diſſipating ſurfaces.

The arguments from the general law ſhew that the velocity of undulation is greateſt in rare mediums.

In applying the general law of interference to theſe colours, as well as to thoſe of thin plates already known, I muſt confeſs that it is impoſſible to avoid another ſuppoſition, which is a part of the undulatory theory, that is, that the velocity of light is the greater, the rarer the medium; and that there is alſo a condition annexed to the explanation of the colours of thin plates, which involves another part of the ſame theory, that is, that where one of the portions of light has been reflected at the ſurface of a rarer medium, it muſt be ſuppoſed to be retarded one half of the appropriate interval, for inſtance, in the central black ſpot of a ſoap-bubble, where the actual lengths of the paths very nearly coincide, but the effect is the ſame as if one of the portions had been ſo retarded as to deſtroy the other.

The central black ſpot on a ſoap bubble is produced by undulations reflected from the confines of a denſe and a rare medium in circumſtances to deſtroy each other:

From conſidering the nature of this circumſtance, I ventured to predict, that if the two reflections were of the ſame kind, made at the ſurfaces of a thin plate, of a denſity intermediate between the denſities of the mediums containing it, the effect would be reverſed, and the central ſpot, inſtead of black, would become white; and I have now the pleaſure of ſtating, that I have fully verified this prediction, by interpoſing a drop of oil of ſaſſafraſs between a priſm of flint-glaſs and a lens of crown glaſs: the central ſpot ſeen by reflected light was white, and ſurrounded by a dark ring. It was however neceſſary to uſe ſome force, in order to produce a contact ſufficiently intimate; and the white ſpot differed, even at laſt, in the ſame degree from perfect whiteneſs, as the black ſpot uſually does from perfect blackneſs.

a contrary effect is produced when both reflections are from a rarer medium.

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Difperfion of colours by refraction explained from the affumptions that light paffes through the particles and through the pores.

The colours of mixed plates fuggeft to me an idea which appears to lead to an explanation of the difperfion of colours by refraction, more fimple and fatisfactory than that which I advanced in the laft Bakerian lecture. We may fuppofe that every refractive medium tranfmits the undulations conftituting light in two feparate portions, one paffing through its ultimate particles, and the other through its pores; and that thefe portions re-unite continually, after each fucceffive feparation, the one having preceded the other by a very minute but conftant interval, depending on the regular arrangement of the particles of a homogenous medium. Now, if thefe two portions were always equal, each point of the undulations refulting from their re-union, would always be found half way between the places of the correfponding point in the feparate portions; but, fuppofing the preceding portion to be the fmaller, the newly combined undulation will be lefs advanced than if both had been equal, and the difference of its place will depend, not only on the difference of the length of the two routes, which will be conftant for all the undulations, but alfo on the law and magnitude of thofe undulations; fo that the larger undulations will be fomewhat further advanced after each reunion than the fmaller ones, and, the fame operation recurring at every particle of the medium, the whole progrefs of the larger undulations will be more rapid than that of the fmaller; hence the deviation, in confequence of the retardation of the motion of light in a denfer medium, will of courfe be greater for the fmaller than for the larger undulations. Affuming the law of the harmonic curve for the motions of the particles, we might without much difficulty reduce this conjecture to a comparifon with experiment; but it would be neceffary, in order to warrant our conclufions, to be provided with very accurate meafures of the refractive and difperfive powers of various fubftances, for rays of all defcriptions.

Comparifon of Dr. Wollafton's obf. with the feparation of colours by thin plates.

Dr. Wollafton's very interefting obfervations would furnifh great affiftance in this inquiry, when compared with the feparation of colours by thin plates. I have repeated his experiments on the fpectrum with perfect fuccefs, and have made fome attempts to procure comparative meafures from thin plates; and I have found that, as Sir Ifaac Newton has already obferved, the blue and violet light is more difperfed by refraction, than in proportion to the difference of the appropriate dimenfions

dimenſions deduced from the phenomena of thin plates. Hence it happens, that when a line of the light proceeding to form an image of the rings of colours of thin plates, is intercepted by a priſm, and an actual picture is formed, reſembling the ſcale delineated by Newton from theory, for eſtimating the colours of particles of given dimenſions, the oblique ſpectrums, formed by the different colours of each ſeries, are not ſtraight, but curved, the lateral refraction of the priſm ſeparating the violet end more widely than the red. The thickneſs correſponding to the extreme red, the line of yellow, bright green, bright blue, and extreme violet, I found to be inverſely as the numbers 27, 30, 35, 40, and 45, reſpectively. In conſequence of Dr. Wollaſton's correction of the deſcription of the priſmatic ſpectrum, compared with theſe obſervations, it becomes neceſſary to modify the ſuppoſition that I advanced in the laſt Bakerian lecture, reſpecting the proportions of the ſympathetic fibres of the retina; ſubſtituting red, green, and violet, for red, yellow, and blue, and the numbers 7, 6, and 5, for 8, 7, and 6.

Subdiviſion of the light of a candle explained.

The ſame priſmatic analyſis of the colours of thin plates, appears to furniſh a ſatisfactory explanation of the ſubdiviſion of the light of the lower part of a candle: for, in fact, the light tranſmitted through every part of a thin plate, is divided in a ſimilar manner into diſtinct portions, increaſing in number with the thickneſs of the plate, until they become too minute to be viſible. At the thickneſs correſponding to the ninth or tenth portion of red light, the number of portions of different colours is five; and their proportions, as exhibited by refraction, are nearly the ſame as in the light of a candle, the violet being the broadeſt. We have only to ſuppoſe each particle of tallow to be, at its firſt evaporation, of ſuch dimenſions as to produce the ſame effect as the thin plate of air at this point, where it is about $\frac{1}{10000}$ of an inch in thickneſs, and to reflect, or perhaps rather to tranſmit, the mixed light produced by the incipient combuſtion around it, and we ſhall have a light completely reſembling that which Dr. Wollaſton has obſerved. There appears to be alſo a fine line of ſtrong yellow light, ſeparate from the general ſpectrum, principally derived from the moſt ſuperficial combuſtion at the margin of the flame, and increaſing in quantity as the flame aſcends. Similar circumſtances might undoubtedly be found in other caſes of the production or modification of light; and experiments upon this ſubject might

might tend greatly to eftablifh the Newtonian opinion, that the colours of all natural bodies are fimilar in their origin to thofe of thin plates; an opinion which appears to do the higheft honour to the fagacity of its author, and indeed to form a very confiderable ftep in our advances towards an acquaintance with the intimate conftitution and arrangement of material fubftances.

Difperfive powers of the eye.

I have lately had an opportunity of confirming my former obfervations on the difperfive powers of the eye. I find that at the refpective diftances of 10 and 15 inches, the extreme red and extreme violet rays are fimilarly refracted, the difference being expreffed by a focal length of 30 inches. Now the interval between red and yellow is about one-fourth of the whole fpectrum; confequently, a focal length of 120 inches expreffes a power equivalent to the difperfion of the red and yellow, and this differs but little from 132, which was the refult of the obfervation already defcribed. I do not know that thefe experiments are more accurate than the former one; but I have repeated them feveral times under different circumftances, and I have no doubt but the difperfion of coloured light in the human eye is nearly fuch as I have ftated it. How it happens to be no greater, I cannot at prefent undertake to explain.

X.

A Memoir on the Wax Tree of Louifiana and Pennfylvania. By CHARLES LOUIS CADET, *of the College of Pharmacy at Paris* *.

Wax bearing plants.

A GREAT number of plants, fuch as the *croton febiferum,* the *tomex febifera* of *Loureiro,* the poplar, the alder, the pine, and feveral labiated plants, afford a concrete inflammable matter by decoction, more or lefs refembling tallow or wax, that is to fay, a fixed oil faturated with oxigen. The light matter, which is called the down of fruits, which filvers the furface of prunes and other ftone fruits, is wax, as Mr. Prouft has fhewn. But the tree which prefents this fubftance

* Tranflated from the Ann. de Chimie, XLIV. 140.

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The moſt productive is the myrica cerifera.

in the greateſt abundance, and in more reſpects than one, is entitled to the attention of cultivators, is the *myrica cerifera*, or wax tree.

Its early hiſtory.

We read in the Hiſtory of the Academy of Sciences for the years 1722 and 1725, that M. Alexandre, ſurgeon, correſpondent with M. Marian, had obſerved at Louiſiana a tree of the ſize of a cherry tree, having the appearance of the myrtle, and bearing a grain of the ſize of coriander ſeed. Theſe grains, of a grey aſh colour, contain a ſmall round hard kernel, which is covered with a ſhining wax that may be obtained by boiling the grains in water. This wax is drier and more friable than ours. The inhabitants make candles of it. M. Alexandre adds, "This grain is uſually of a deep and beautiful lake colour, which by merely cruſhing with the fingers leaves them tinged, but this is only at a particular ſeaſon."

The liquor in which the grain has been boiled, and from whence the wax is procured, having been poured out and evaporated to the conſiſtence of an extract, M. Alexandre diſcovered that it checks the moſt obſtinate dyſenteries.

The advantageous properties that this tree appears to poſſeſs, ought to have induced philoſophers to make enquiries to aſcertain the various properties of the vegetable, and what attention its culture might require; it has been long conſidered merely as an object of curioſity.

Linnæus, in his vegetable ſyſtem, only mentions the wax tree of Virginia, myrica cerifera, the leaves lanceolated as if dentated, and the ſtem arboreſcent.

Species of the myrica cerifera.

On enquiring of Cit. Ventenat, whether there were ſeveral ſpecies, he informed me, that Ayton diſtinguiſhes two, viz.

1. The *myrica cerifera anguſtifolia*, which grows in Louiſiana. This tree is very delicate, it flowers with difficulty in our green houſes; and its grains are ſmaller than thoſe of the following.

2. The *myrica cerifera latifolia*, which grows in Penſylvania, Carolina, and Virginia, does not riſe ſo high as the former; it is perfectly naturalized in France. Theſe two myrica are diæcous.

They are both cultivated in the *Muſéum des Plantes*, and in the gardens of Citizens Cels and Lemonier (at Paris.)

Cit.

Cit. Michault admits of a third ſpecies of *myrica cerifera,* which he calls the dwarf wax tree. Cit. Ventenat believes that wax may be procured from all the myrica.

The authors who have mentioned theſe trees with ſome detail are Marſhal, tranſlated (into French) by Leſerme, Lepage-Duprat, and Toſcan, librarian at the Muſeum of Natural Hiſtory. A memoir inſerted by the laſt in his work intitled *L'Ami de la Nature,* ſhews the manner in which the vegetable wax is collected in the colonies. **Authors who have written reſpecting it.**

" Towards the end of autumn," ſays he, " when the berries are ripe, a man leaves his houſe, together with his family, to go to ſome iſland or bank on the ſea ſhore where the wax trees grows in abundance. He carries with him veſſels to boil the berries, and a hatchet to build a cottage where he may find ſhelter during his reſidence in this place, which is uſually three or four weeks. While he cuts down the trees his children gather the berries. A very fertile ſhrub will afford near ſeven pounds. When theſe are gathered the whole family employ themſelves in procuring the wax. They throw a certain quantity of berries into the kettle, and then pour a ſufficient quantity of water on them ſo as to cover them to a depth of about half a foot. They boil the whole, ſtirring the grains about and rubbing them againſt the ſides of the veſſel, in order that the wax may more eaſily come off. In a ſhort time it floats on the water like fat, and is collected with a ſpoon and ſtrained through a coarſe cloth to ſeparate it from any impurities which might be mixed with it. When no more wax can be obtained, they take the berries out with a ſkimmer, and put others into the ſame water; but it muſt be entirely changed the ſecond or third time, and in the mean time boiling water muſt be added as it evaporates, in order to avoid retarding the operation. When a conſiderable quantity of wax has been obtained by this means, it is laid upon a cloth to drain off the water with which it is ſtill mixed. It is then dried and melted a ſecond time, to render it more pure, and it is then formed into maſſes. Four pounds of berries afford about one of wax; that which is firſt obtained is generally yellow: but in the latter boilings it aſſumes a green colour from the pellicle with which the kernel of the berry is covered." **Manner of collecting the wax in America.**

The

It is there ufed for foap making.

The traveller Kalm, fpeaking of the vegetable wax, fays, that in the country where the wax tree grows they make excellent foap of it, which wafhes linen perfectly white.

The author obtains fome of the wax;

Such are the notions which have been formed refpecting the myrica; at leaft no other obfervations had been publifhed to my knowledge, when a naturalift favoured me with half a kilogramme ($17\frac{1}{2}$ oz. avoird.) of the vegetable wax of Louifiana. I was defirous of making a comparative analyfis of it with bees wax; but before I undertook this work, I wifhed to fee the tree and berry of the myrica. I faw this precious vegetable at the *Jardin des Plantes,* and I wrote to Cit. Defhayes, a zealous botanift, who cultivates the myrica penfylvania at Rambouillet, to requeft him to give me fome information concerning it. He had the politenefs to fend me an anfwer with fome of the berries, which I immediately examined.

and examines the grain.

This grain is a kind of berry of the fize of a pepper corn; the outfide when it is ripe and frefh, is white, and covered with fmall black afperities which give it the appearance of fhagreen. When it is rubbed in the hands, it renders them greafy or unctuous.

Experiments to feparate the waxy part mechanically.

If one of thefe little berries be ftrongly preffed, it parts with a matter refembling ftarch, and mixed with fmall brown round grains like fine gunpowder. The nut which remains bare has a very thick ligneous covering, and contains a dycotildonous kernel. By rubbing a handful of the berries on a fieve of horfe hair, I obtained a grey powder, in which the eye diftinguifhes without the affiftance of a magnifying glafs, the fmall brown grains I mentioned, mixed with a white powder.

Application of alcohol.

I put this powder into alcohol, which with the affiftance of a flight heat, diffolved all the white part, and left the black powder which I feparated. Water poured on this folution in alcohol formed a precipitate that floated on the furface of the liquid. I melted this and obtained a yellow wax refembling that fent me from Louifiana. This experiment compleatly proves that the wax of the myrica is the white grumous matter that furrounds the berries.

Black powder from the furface of the berries.

The black powder which was feparated appeared to me to contain the colouring principle, and I hoped alfo to find in it

it the beautiful lake mentioned by M. Alexandre. With this notion I crufhed the powder ftrongly, and boiled it in a folution of acid fulphate of alumine; but was very much furprifed to obtain nothing but a liquor fcarcely coloured, and by which the alumine, when precipitated by an alcali, was but flightly tinged.

I took another portion of this fine black powder, and infufed it in alcohol, I foon obtained a tincture of the colour of wine lees, which on being heated, became as red as a ftrong tincture of quinquina, or terra japonica. The refult led me to think that the colouring principle was refinous; but on adding water to it, I did not perceive that any precipitate was formed.

I then poured into this tincture water charged with fulphate of alumina, and obtained flight precipitate. A folution of fulphate of iron immediately formed ink.

The powder afforded an aftringent folution by alcohol.

What may this aftringent colouring principle be which is only foluble in alcohol, which is not precipitated by water, and has fo little attraction for alumine? To difcover it, it is neceffary to make a courfe of experiments which the fmall quantity of the fubftance I was in poffeffion of would not permit me to undertake. The aftringent matter mentioned by M. Alexandre muft be found in the decoction of the entire grain. To afcertain this fact I boiled fome in a filver faucepan, the decoction upon which floated a little of the wax, was of a greenifh colour, its tafte flightly ftyptic, and it precipitated black ferruginous folutions. I heated it in an iron veffel for that purpofe, and it immediately became black. To difcover whether this property arofe from the gallic acid alone, or from tanin, I mixed a little of the concentrated decoction with a folution of gelatine, but it afforded no precipitate.

It contained gallic acid but no tanin,

It is therefore to the confiderable quantity of gallic acid which the berries of the myrica contain, that we muft attribute its effect in dyfenteries. I therefore fuppofe that the bark and leaves of the tree muft contain an extract much more aftringent than the berries.

Examination of the wax.

The examination of the wax afforded extremely interefting refults.

In whatever manner the wax may be obtained; by the decoction of the grains, or the folution of the powder when precipitated from alcohol by water, this wax when melted is al-

2 ways

It is greenish yellow; firm, and more brittle than bees wax, burns well in candles, and emits a fragrant smell.

ways of a greenish yellow. It is of a firmer consistence than bees wax, it is dry, and sufficiently friable to be pulverized. In a word, it is evidently more oxigenated than the wax prepared by bees. Candles made of the wax of the myrica afford a white flame, a good light, without smoke, and do not gutter; they emit when quite fresh a balsamic odour, which the inhabitants of Louisiana consider as extremely wholesome for persons in ill health. When distilled in a retort this wax passes over for the most part in the form of butter. This portion is much whiter, and has no more consistence than tallow. Another portion that was decomposed afforded a little water, with some empyreumatic oil, and sebacic acid. Much carbonated hidrogen gas, and carbonic acid gas were disengaged; there remained in the retort a black and coaly bitumen. Wax usually is affected in the same manner by distillation.

In distillation it is affected like bees wax.

Ether dissolves it better than alcohol.

I have already mentioned, that alcohol dissolves the wax of the myrica, but ether dissolves it better, and it separates in the form of stalagmites by the evaporation of the liquid; neither of them discolour it. If this wax be boiled with weak sulphuric acid it becomes paler, but there is no evident combination of the acid with it. The yellow bees wax under the same circumstances did not change colour.

Ox. mur. acid bleaches it.

Oxigenated muriatic acid renders both kinds of wax perfectly white. The vegetable wax is the most difficult to be bleached.

It is soluble in ammonia;

Vegetable wax dissolves in ammonia: the solution is of a brown colour; a portion of the wax is saponified. Volatile alkali has much less action on bees wax.

and forms soap with fixed alkali.

Both kinds of wax when strongly agitated in a boiling solution of caustic potash, become white and form a real soap as Kalm observed.

Its whiteness in soap.

The whiteness acquired by the wax during this saponification is not a new phenomenon. Chaptal in his process for bleaching by the vapor of alkaline leys, proves that the colouring principle of vegetables yields to the action of alkalis. Some chemists have attributed this effect to the direct combination of the soda or potash with the coloured extract, a combination which makes it almost of a soapy quality, and renders it soluble.

Chemical observation on the oxigenation of the oil in soap.

I apprehend that in this operation, the alkali exercises a double attraction on the oil or wax, first directly with the con-

stituent

ſtituent principles of the oil and then prediſpoſing, and favouring the combination of the atmoſpheric oxigen with the oil or wax. I do not know whether my notion is original; but I deduced it from the obſervation of what paſſes during the decompoſition of a ſoap by an acid: the oil is always concrete and becomes oxigenated. It would be intereſting for chemical theory to make, if it were poſſible, a ſoap in a cloſed apparatus the air of which might be examined after the experiment, or in different gaſes not containing oxigen.

When the ſoap of myrica is decompoſed a very white wax is obtained, but in a peculiar ſtate rendering it unfit for our uſes. The veg. wax from its ſoap is in a peculiar ſtate.

Litharge or the ſemi-vitreous oxide of lead diſſolves very well in melted wax of Louiſiana; it forms a very hard plaiſter, but its conſiſtence may be diminiſhed at pleaſure by the addition of a little oil. If as there is reaſon to ſuppoſe, the wax of myrica retains a portion of the aſtringent principle afforded by the decoction of the berries, phyſicians may perhaps diſcover ſome uſeful topical remedies in the compounds of this wax. Plaiſters with veg. wax.

From what has been ſaid, we ſee that the myrica may be of the greateſt uſe to the arts. The wax which it affords is ſufficiently abundant to recompenſe the care and expenſe of cultivating it. For a buſh in a full bearing yields from ſix to ſeven pounds of kernels, one fourth of which may be obtained in wax. It is ſuperior in quality to bees-wax. Great utility of this wax.

The aſtringent principle of the myrica, extracted in the large way may be very uſeful in medicine and in the arts; it may to a certain extent be ſubſtituted inſtead of nut galls in dyeing, hat making, and probably in certain proceſſes of tanning. The colouring principle appears ſufficiently ſolid to deſerve ſome attention; and if it be true that ſome fine lakes have been obtained from it in Louiſiana, why may we not expect advantages from it in painting. and of its aſtringent matter.

Laſtly when this wax ſhall have become plentiful and cheap in the market it promiſes great advantage in the fabrication of ſoap.

The art of bleaching this wax will alſo require a courſe of experimental reſearch; if it be propoſed to operate economically and in the large way. Two re-agents preſent themſelves to manufacturers; the ſulphuric acid, and the oxigenated muriatic To bleach veg. wax.

muriatic acid. But as the wax does not ſink in theſe liquids, we muſt multiply the contacts either by ſlicing and ſprinkling it with the oxigenated muriatic acid, or by incloſing it in this divided ſtate in caſks into which the oxigenated muriatic gas ſhould be paſſed.

The preference given to the action of the ox. mur. of lime.

I ſhall propoſe a third which promiſes a ſpeedier effect. The wax in a very divided ſtate is to be ſtratified in a caſk, with ſuper-oxigenated muriate of lime; in this manner they are to be diſpoſed in ſtrata, and left for ſome time in contact without moiſture. The ſalt is afterwards to be decompoſed with water acidulated by the ſulphuric acid; taking care to pour the water a little at a time at different intervals, until there ſhall be no longer any perceptible diſengagement of muriatic gas; at which period a large quantity of water is to be added and the mixture agitated with a ſtaff. The inſoluble ſulphate of lime falls down by repoſe, while the bleached wax riſes and ſwims at the ſurface. This is to be waſhed and melted on the water bath.

On the culture of the tree in Europe.

I ſhall conclude this memoir by offering ſome notions reſpecting the culture of the *myrica penſylvanica*.

Cit. Deſhayes to whom I am indebted for the trials I have made has obſerved the wax tree for ſeveral years at Rambouillet. The following is what he writes to me on the ſubject.

It is perfectly at home in France.

" The *myrica (latifolia)* Ayton is here abſolutely in its native country; it is in the ſoil beſt ſuited to it, namely in a ſandy and blackiſh turf: we have ſixteen very flouriſhing wax trees. Their height is from four to five and ſix feet, and one of them a male is ſeven feet high. The berries are abundant almoſt every year, I ſay almoſt becauſe there are ſome years in which they have failed. In general this fruit thrives very well in the part of the Engliſh garden aſſigned to it.

and requires no care in its culture.

" The culture requires no care. Every year numerous ſhoots are taken off which riſe at the feet of the large wax trees. Theſe are planted in other parts of the garden at the diſtance of one metre or yard aſunder."

The grain may be ſowed in the ground in ſpring, and afterwards tranſplanted, but the proceſs would be too long. The myrica ſucceeds wherever the ſoil is light and rather moiſt. How many provinces are there in which this cultivation might become uſeful and give employment to lands at preſent nearly abandoned.

How great are the advantages our agriculture might expect from this acquisition, since the myrica has long ago been seen to flourish in the dry sands of Prussia. Cit. Thieboult of the academy of Berlin, has communicated this interesting fact to me in the following note. It even grows well in Prussia.

The late Mr. Sulzer author of the general dictionary of the fine arts, obtained from Frederic the Great a portion of waste ground of considerable extent on the banks of the Spree, half a league from Berlin, in a place called the Moabites. However ungrateful this soil might appear as it presented only a very scanty and thin turf, upon a fine and light sand, Mr. Sulzer converted it into a garden very agreeable and worthy of a philosopher. Among other remarkable things, he made a plantation of foreign trees composed of five long rows, in the direction from East to West. There were not two trees in succession of the same species; he had placed in the rows most exposed to the North, none but such as were loftiest and most capable of resisting the rigours of the climate, So that by proceeding from North to South, the first row presented only trees of about seventy feet in height, the second trees between twenty-five and thirty feet high, and so in succession in an amphitheatre, where all the trees enjoyed the sun, at least in a part, and the weakest were sheltered by those which were more hardy. Narrative of a plant in the garden of Mr. Sulzer.

It was in the southernmost row, that I observed a kind of bush only two or three feet high, which Mr. Sulzer called the wax tree. All the visitors took particular notice of this tree in preference to all others, on account of the delicious odour of its leaves, which they preserved a very long time.

Citizen Thieboult afterwards speaks of the extraction of the wax. This operation does not differ from that related by Mr. Alexander.

I have seen, adds he afterwards, a single candle of this wax perfume the three chambers which composed the particular apartment of Mr. Sulzer, not only during the time it was lighted, but also for the rest of the evening. Remarkable perfume from the candles of veg. wax.

Without doubt the myrica cultivated at Berlin, was more odoriferous than that which grows with us, for ours does not emit the same perfume. Mr. Sulzer had the project of making candles of this wax, not bleached, but covered with our wax for the sake of beauty. The heirs of the academician have

 sold

ſold the garden, but the wax tree ſtill remained. It was planted in 1770. Since the poſſibility of naturalizing the *myrica cerifera* has been aſcertained in the north, why ſhould we neglect a vegetable of ſuch value and importance which could not fail to proſper in our ſouthern departments, and demands much leſs care than our bee-hives. The ſucceſsful experiments already made ought to excite the zeal of our cultivators.

Economical Remarks.

The government has already encouraged this branch of induſtry, by ordering plantations to be made. There exiſts at Orleans and at Rambouillet, two orchards of the wax tree which contain more than four hundred ſhrubs. We cannot give too much publicity to ſuch ſatisfactory reſults. Nothing is propagated with ſo much ſlowneſs as uſeful plants. A barren but pictureſque tree, or an agreeable flower are ſoon adopted by the faſhion. They ornament the gardens of our modern Lucullufes, and the toilets of our Phrynes, while our indefatigable agriculturiſts make vain efforts to enrich our gardens with a new gramineous plant, or to fill our barns with nouriſhing cereal plants. The people has long rejected from prejudice both maize and the potatoe which have been ſo highly ſerviceable to the poor and to our ſoldiery. We no longer find in our foreſts the food bearing oak, upon which our anceſtors ſubſiſted. Let us hope that our cultivators will at laſt open their eyes upon their true intereſts, and that leſs enſlaved by old practices they will not deſpiſe the preſents which learned ſocieties are deſirous of making for their profit, and the reputation and proſperity of their country.

Outline

XI.

Outline of the Craniognomic Syſtem of Dr. Gall, Phyſician at Vienna. By Dr.* BOJAMES †.

THE deſire of finding in the external ſtructure of man, certain indications of his internal faculties, his paſſions, his moral diſpoſition, &c. has in the moſt remote as well as in modern times, engaged philoſophers to eſtabliſh ſyſtems of phyſiognomy, which have been more or leſs ſatisfactory.

Various ſyſtems of phyſiognomy,

The moſt generally known are thoſe of Porta, Lavater, the theory of the facial angle, and laſtly the ſyſtem of Gall.

of Porta, of Lavater, and of the facial angle.

With regard to the firſt, who has buſied himſelf in comparing the outlines of the figure of man, with that of brutes, obſervers have decided on its value, and conſider his principles as the product of a wild imagination; they have found them too haſty, very little eſtabliſhed on reaſonable obſervations, and abſolutely uncertain in application.

Syſtem of Porta, haſty and inaccurate.

Lavater's ſyſtem has had more ſucceſs; but though we revere the genius of this man who was really a great obſerver, we cannot be ignorant of the looſe foundation on which all his opinions are built, and the mind is unſatisfied with truths which can only be appreciated by an imagination as exalted, and feelings as delicate as thoſe of the author.

Lavater's ſyſtem founded on ſentiment, not ſcience.

The theory of the facial angle which comprehends a more ample field than the ſyſtem of Lavater, leaves us in uncertainty as to the detail of faculties, and gives us only general points of view. But it preſents this moſt important truth, that the facial angle increaſes in magnitude in equal proportion with the faculties of animals, and in this point it evidently agrees with the general reſults of Gall's ſyſtem.

Theory of the facial angle true; but too general.

Without entering into a ſcrupulous detail of the laborious courſe which this learned philoſopher has followed, in order to

Syſtem of Gall its fundamental principles.

* This hiſtorical expoſition which does not in the leaſt tend to prove the truths of Gall's ſyſtem, ſhould not influence any one's judgment concerning it, as it will be confirmed by its author with ſolid reaſoning and convincing proofs.

It is likewiſe neceſſary to remark, that the ſentences marked with inverted commas do not reſt on the authority of Gall.

† Inſerted by him in the Encyclopedie Methodique, of the learned *Millin*.

to eſtabliſh certain baſes for a ſcience hitherto ſo hypothetical, I ſhall content myſelf with a ſhort examination of its fundamental principles, which are,

1ſt. *The Brain is the material Organ of the Internal Faculties.*

The brain is the organ of the faculties.

Without endeavouring to decide upon the metaphyſical queſtions reſpecting the nature of the ſoul, or of that which may be ſuppoſed the occult cauſe of the internal faculties, we are nevertheleſs compelled to admit a material organ for their action.

Proofs.

Now when we remark that theſe faculties are found only where the brain exiſts, that they are loſt with it; that the diſorders and injuries ſuſtained by this organ, very ſenſibly influence their degree and their action; that the volume of the brain increaſes in direct proportion with the faculties of animals, &c. when we obſerve all this I ſay, there is nothing of conjecture in ſuppoſing the brain to be their material and intermediate organ.

Note. It might here be objected that in many caſes individuals have loſt a conſiderable portion of the ſubſtance of the brain, without their faculties having been ſenſibly diminiſhed; but it muſt be obſerved, that in general the organs of the brain are double, and that the caſes are far from being accurately ſtated or eſtabliſhed.

The brain conſiſts of independent organs.

2d. *The Brain contains different Organs independent* * *of each another, for the different Faculties.*

The internal faculties do not always exiſt in equal proportions with reſpect to each other; there are men who have much intelligence without much memory, courage without circumſpection, and metaphyſical genius without being profound obſervers.

Proofs. The faculties are exerted independently of each other, &c.

Again the phenomena of dreams, of ſomnambuliſm, of madneſs, &c. prove that the internal faculties do not always act together, that ſome are often extremely active, while others are at the ſame time totally inſenſible.

* This notion of independence does not deſtroy the principle of animal organization, that all the parts have a mutual relation; it only indicates that the action of one organ does not abſolutely cauſe the ſame degree in another. B.

Thus

Thus in old age, and ſometimes in diſeaſes, madneſs for example, many faculties are loſt whilſt others ſubſiſt; and conſtant employment of the ſame faculty ſenſibly diminiſhes its energy; if we paſs to another we find it has all the force of which it is ſuſceptible, and when we again employ the firſt faculty, we find it has recovered its original vigour. Thus it is that when fatigued with an abſtract and philoſophical reading, we turn with pleaſure to poetry, and then afterwards apply again with equal attention to our former employ.

Theſe phenomena prove that the faculties are ſeparated and independent of one another, and we are induced to believe that the ſame is the caſe with their material organs.

Note. "We do not entirely adopt this notion of Gall, but on the contrary, we believe that the ſeparation of the material organs muſt be conſidered as the cauſe of the diſtinction of the internal faculties; at leaſt it ſeems to us, that by ſuppoſing them originally ſeparated, we cannot avoid the ſnare of the materialiſm which exiſts as ſoon as we ceaſe to conſider the ſpirit (l'eſprit) as unity." **Theological note.**

3d. *The Developement of the Organs contained in the Cranium, is in direct Proportion with the Force of their correſponding Faculties.* **The organs of the brain are more developed the ſtronger the correſpondent faculties,**

This principle dictated by analogy, depends on the axiom; that through all nature the faculties are found to be ever in proportion to their relative organs, and its truth is eminently proved by the particular obſervations of Gall.

It muſt however be obſerved, that exerciſe has a conſiderable influence on the force of the faculties, and that an organ but moderately developed, but which is often exerciſed, may afford a ſuperior faculty to that which accompanies an organ of great magnitude, but is never put in action. Thus we ſee men whoſe ſtructure is but moderately ſtrong, acquire by continual exerciſe powers ſuperior to others whoſe ſtructure is almoſt athletic. **and the more from exerciſe.**

Note. I muſt here anticipate an opinion which ſeems to reſult immediately from this principle, but which is nevertheleſs falſe, that the volume of the brain is in direct proportion to the energy of its faculties. Obſervation has demonſtrated to Gall, that the power of the faculties can only be appreciated by the developement of the organs ſeparately, which form **A large brain does not indicate energy of faculties unleſs the organs be ſeparately developed.**

form diſtinct eminences on the cranium, and that a cranium perfectly round, whatever may be its magnitude, never exhibits faculties either great or numerous.

Conjecture as to the reaſon.

I do not recollect that I ever heard Gall give his reaſons for the concluſion: "but I think we may conſider theſe heads in a ſtate ſimilar to that of obeſity, and as we do not judge of the muſcular ſtrength of a man or animal by the volume of its members, but by their particular developement, ſo I think we muſt judge of the force of the faculties by the developement of their reſpective organs."

The external figure of the cranium exhibits the organs and ſhews the power of the faculties, becauſe the form of the cranium depends on that of the brain.

Laſtly, the fourth principle which is the moſt important in the practice of the ſyſtem of Gall, is: *That we may judge of theſe different organs and their faculties by the exterior form of the cranium.*

The truth of this principle is founded on another; namely, that the formation of the cranium depends on that of the brain, a truth generally known and proved by the anteriority of the brain, and by the impreſſions or indentations within the cranium.

Remark.

Note. It is true that there are craniums in which there is an internal protuberance of the bone correſponding with the external projection; and this irregularity, which is ſometimes found to be a diſorder, moſt commonly in an advanced age, when the organs of the brain do not ſo powerfully reſiſt the cranium, throws a degree of uncertainty in the practice of the ſyſtem of Gall; but this is the fate of all our truths which are dictated by experience: theſe caſes, however, are not very frequent.

On theſe principles Gall has compared the organs of men and animals.

Guided by theſe principles Gall has examined nature; he has compared the craniums of men and animals, and thoſe of men of ſimilar and of different faculties. His reſearches have almoſt inconteſtibly proved, not only the truths above ſtated, but that the faculties of animals are ſimilar to thoſe of man; that what we call inſtinct in animals is alſo found in man; for example, love, cunning, circumſpection, courage, &c. that the quantity of organs is the ſtandard which fixes the generic difference of animals; their mutual proportion, that of the individuals; that the diſpoſition to any faculty which is originally given by nature, may be developed by exerciſe and favourable circumſtances, and ſometimes by diſorders; but that it can

never be created if not given by nature *; that the accumulation of organs is conftantly made from back to front, and from below upwards, fo that animals in proportion as they refemble man in the quantity of their faculties, have the fuperior and anterior part of the cranium more developed; and laftly in man, the moft perfect of animals, there are organs in the fuperior and anterior parts of the frontal and parietal bones affigned to faculties which exclufively belong to him. "In this laft point of view it is that the difcoveries of Gall perfectly coincide with the theory of the facial angle, which appears to confirm their truth."

It is difficult to give an accurate and fatisfactory account of the detail of this fyftem, and of the various organs which Dr. Gall has difcovered, without ftating the variety of facts and examples which he exhibits as evidence to prove what he advances; I will however undertake this enumeration, being perfuaded that it will in many refpects fhew the author's manner of reafoning, and give a clear notion of the manner of proceeding required to obtain his refults †. Detail of the fyftem.

1. *The Organ of Tenacity of Life.*

The firft organ which the author thinks he has difcovered is that of the tenacity of life, *tenacitas vitæ;* he fuppofes the *medulla oblongata* to be its feat; and as the circumference of the great aperture of the *occiput* is in direct proportion to the extent of the *medulla oblongata,* he judges of the intenfity of the life of an animal by the magnitude of this perforation. Organs. 1. Tenacity of life.

The obfervations which fupport this opinion are, that the perforation is generally larger in the *cranium* of women than men; that it is conftantly of greater extent in the cat, the otter, the beaver, the badger, &c. animals which are known to be extremely tenacious of life. And the moft fpeedy method of killing an animal is to divide the fpinal marrow.

2. *Organ of Inftinct for felf-prefervation.*

Forwarder than the *medulla oblongata* at the place where it quits the brain, the author fuppofes the *organ of love of life, or inftinct for felf-prefervation* to exift. 2. Self-prefervation.

* It is neceffary that the germ of any organ fhould fubfift in the embryo, in order to its fubfequent developement as an organ.

† Compare the correfponding numbers with thofe of Plate X.

As

As animals do not afford examples of ſuicide, he could only obtain proofs of this ſuppoſition in the human race, and ſeveral caſes of voluntary ſuicide in which this part of the brain was diſordered, have induced him to conſider it as the organ of this faculty; he does not however admit it as indiſputably eſtabliſhed, but waits for further examples to confirm the fact.

3. *Organ of the choice of Food.*

3. Choice of food.

The author ſuppoſes the organs for the choice of food to be placed in the quadrijumal tubercles, the anterior of which are greater in carnivorous animals, the poſterior developed in the herbivoran, but are of equal ſize in the omnivorous animals.

4. *Cerebral Organs of the external Senſes.*

4. External ſenſes.

The middle part of the baſe of the brain is appropriated to the external ſenſes. It is the region from which thoſe nerves iſſue which are diſtributed into the organs of thoſe ſenſes.

(To be continued.)

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Account of the Ventriloquiſm exhibited by M. Fitz-James.

Account of ventriloquiſm.

THE reader will doubtleſs recollect ſome curious facts and obſervations on ventriloquiſm by Mr. Gough in our ſecond volume, page 125. That acute philoſopher, reaſoning from a few facts and ſtating the want of more, appears inclined to adopt the theory that the art of ventriloquiſm conſiſts in cauſing the voice to iſſue from the mouth only, and uttering it in ſuch a direction, that the hearer may receive the impreſſion of ſome echo with conſiderably more force than he receives the original ſound. He gives inſtances of this proceſs, particularly where the ſound of a ring of bells appears to change its direction accordingly, as the hearer by moving along, receives it from different reflecting ſurfaces, while the original ſound is interrupted by ſome obſtacle. Whether the echos in a room be at all likely to be ſo managed would admit of conſiderable doubt; and without having witneſſed any exhibition of this kind in the leaſt to be compared with the ſurprizing narratives we occaſionally hear, I have always been ſtrongly diſpoſed

difpofed to think that the delufions with regard to the fuppofed direction of the voice in fuch cafes are not phyfical but moral; that is to fay, they have arifen from fomething in the nature of the fubject, or the pofition and action of the fpeaker, with the character, tone, and manner of fpeaking. On the prefent occafion I have the fatisfaction to give fome account of the performance of M. Fitz. James, one of the firft mafters of this art; who in addition to his very ftriking powers as a fpeaker and actor has the candour and liberality to explain the nature of his performance to his auditors. I was prefent a few evenings ago at a public exhibition, which continues to be repeated at *Dulau's* in Soho-fquare; and though my account of what I faw and heard cannot but be very imperfect, and far from exciting the furprize which the actual performance produces, it may neverthelefs be of utility to eftablifh a few principles, and remove fome errors refpecting this art. Account of ventriloquifm.

After a comic piece had been read by Monf. Volange, M. Fitz-James, who was fitting among the audience, went forward and expreffed his fufpicion, that the ventriloquifm was to be performed by the voices of perfons concealed under a platform which was covered with green cloth. Replies were given to his obfervations apparently from beneath that ftage; and he followed the voices with the action and manner of a perfon whofe curiofity was much excited, making remarks in his own voice, and anfwering rapidly and immediately in a voice which no one would have afcribed to him. He then addreffed a buft which appeared to anfwer his queftions in character, and after converfing with another buft in the fame manner, he turned round, and in a neat and perfpicuous fpeech explained the nature of the fubject of our attention: and from what he ftated and exhibited before us, it appeared that by long practice he had acquired the faculty of fpeaking during the infpiration of the breath with nearly the fame articulation, though not fo loud nor fo varioufly modulated as the ordinary voice formed by expiration of the air. The unufual voice being formed in the cavity of the lungs is very different in effect from the other. Perhaps it may iffue in a great meafure through the trunk of the individual. We fhould fcarcely be difpofed to afcribe any definite direction to it; and confequently are readily led to fuppofe it to come from the place beft adapted to what was faid. So that when he went to the

door

Account of ventriloquifm.

door and * afked " Are you there," to a perfon fuppofed to be in the paffage, the anfwer in the unufual voice was immediately afcribed by the audience to a perfon actually in the paffage; and upon fhutting the door and withdrawing from it, when he turned round, directing his voice to the door and faid, " Stay there till I call you," the anfwer which was lower, and well adapted to the fuppofed diftance and obftacle interpofed, appeared ftill more ftrikingly to be out of the room. He then looked up to the cieling and called out in his own voice, " What are you doing above? do you intend to come down?" to which an immediate anfwer was given, which feemed to be in the room above, " I am coming down directly." The fame deception was practifed on the fuppofition of a perfon being under the floor, who anfwered in the unufual but a very different voice from the other, that he was down in the cellar putting away fome wine. An excellent deception of the watchman crying the hour in the ftreet, and approaching nearer the houfe till he came oppofite the window was practifed. Our attention was directed to the ftreet by the marked attention which Fitz-James himfelf appeared to pay to the found. He threw up the fafh and afked the hour, which was immediately anfwered in the fame tone, but clearer and louder; but upon his fhutting the window down again the watchman proceeded lefs audibly, and all at once the voice became very faint, and Fitz-James in his natural voice faid, " he has turned the corner." In all thefe inftances, as well as others, which were exhibited to the very great entertainment and furprize of the fpectators, the acute obferver will perceive that the direction of the found was imaginary, and arofe entirely from the well ftudied and fkilful combinations of the performer. Other fcenes which were to follow required the imagination to be too completely mifled to admit of the actor being feen. He went behind a folding fcreen in one corner of the room, when he counterfeited the knocking at a door. One perfon called from within, and was anfwered by a different perfon from without, who was admitted, and we found from the converfation of the parties, that the latter was in pain, and defirous of having a tooth extracted. The dialogue, and all the particulars of the operation that followed, would require a long difcourfe if I were to attempt to defcribe them to the reader. The imitation of the

* The whole performance was in French.

Account of ventriloquifm.

the natural and modulated voice of the operator encouraging, foothing, and talking with his patient; the confufion, terror, and apprehenfion of the fufferer; the inarticulate noifes produced by the chairs and apparatus, upon the whole, conftituted a mafs of found which produced a ftrange but comic effect. Loofe obfervers would not have hefitated to affert, that they heard more than one voice at a time; and though this certainly could not be the cafe, and it did not appear fo to me, yet the tranfitions were fo inftantaneous without the leaft paufe between them, that the notion might very eafily be generated. The removal of the fcreen fatisfied the fpectators that one performer had effected the whole.

The actor then proceeded to fhew us fpecimens of his art as a mimic; and here the power he had acquired over the mufcles of his face was fully as ftrange as the modulations of his voice. In feveral inftances he caufed the oppofite mufcles to act differently from each other; fo that while one fide of his face expreffed mirth and laughter, the other fide appeared to be weeping. About eight or ten faces were fhewn to us in fucceffion as he came from behind the fcreen, which together with the general habits and gait of the individual totally altered him. In one inftance he was tall, thin, and melancholic; and the inftant afterwards, with no greater interval of time than to pafs round behind the fcreen, he appeared bloated with obefity, and ftaggering with fulnefs. The fame man at one time exhibited his face fimple, unaffected, and void of character, and the next moment it was covered with wrinkles expreffing flynefs, mirth, and whim of different defcriptions. How far this difcipline may be eafy or difficult, I know not, but he certainly appeared to me to be far fuperior to the moft practifed mafters of the countenance I have ever feen.

During this exhibition he imitated the found of an organ; the ringing of a bell, the noifes produced by the great hydraulic machine of Marli, and the opening and fhutting of a fnuff box.

His principal performance, however, confifted in the debates at the meeting of *Nauterre,* in which there were twenty different fpeakers, as is afferted in his advertifement; and certainly the number of different voices was very great. Much entertainment was afforded by the fubject, which was taken

Account of ventriloquifm.

taken from the late times of anarchy and convulfion in France; when the loweft, the moft ignorant part of fociety, was called upon to decide the fate of a whole people by the energies of folly and brute violence. The fame remark may be applied to this debate, as to the other fcene refpecting tooth-drawing; namely, that the quick and fudden tranfitions, and the great differences in the voices gave the audience various notions, as well with regard to the number of fpeakers, as to their pofitions and the direction of their voices.

This account of a very celebrated ventriloquift may perhaps feem too minute and particular for a philofophical Journal, which is lefs devoted to do juftice to the talents of men, than to inveftigate the caufes of things. But where a ftriking delufion may lead to miftaken theories of found, I conceive it to be no fmall part of the argument, which is to fet the truth in a clear light, to fhew how numerous and extraordinary the acquifitions and the ability of the performer may be. There is likewife a point of delicacy arifing from the fufpicion of improper motives, when a public performer is fpoken of in terms of approval by a periodical writer. On this head, however, I feel fo little difficulty, that I am almoft in doubt whether I fhould obliterate the laft obfervation, or fuffer it to pafs.

Afcent of Mont Blanc and Mont Perdu.

Afcent of Mont Blanc by M. Forneret and Dortheren,

M. Forneret of Laufanne, and the Baron de Dortheren, have undertaken a new excurfion to the fummit of Mont Blanc. After two days travel, they arrived at the fummit, where the wind was fo turbulent that they were forced to fit together with their guides in a mafs to prevent their being precipitated. The cold was fix degrees below congelation, and with the rarity of the air affected their lungs in fo painful a manner, that they declare that no inducement fhould prevail on them to repeat this expedition. The enterprize was without any beneficial confequence to the fciences. *Bibliotheque de Sonini.*

by de Sauffure,

It was in the year 1787, that the celebrated De Sauffure, arrived at the fummit of Mont Blanc; and his voyage was confidered as a real conqueft for the natural fciences. Sonini has learned by a private letter, that the indefatigable Ramond, well known from his refearches in natural hiftory, has at laft reached the fummit of Mont Perdu, the giant of the Pyreneans,

of Mont Perdu by Ramond.

neans as Mont Blanc is that of the Alps. This mountain covered with glaciers and eternal ſnows, which is elevated more than eighteen hundred toiſes above the level of the ſea; and riſes higher than all the granitic Pics except Mont Blanc, muſt nevertheleſs be ranked among ternary mountains, ſince it contains the remains of ſea animals and quadrupeds. The efforts of Ramond to reach the ſummit have till now been conſtantly unſucceſsful, and he is the only one who has yet accompliſhed it. It preſents on all ſides threatening projections and ſteep precipices. This time, inſtead of directing his courſe from north to ſouth by aſcending the mountain on the French ſide, he travelled from ſouth to north by the ſlope which is directed towards Spain. He will ſoon publiſh the intereſting recital of his aſcenſion.

Experiment on Sound.

Low Sounds ſaid to be conveyed to great diſtances.

In the Journal laſt quoted, there is an account or notice of an experiment performed in the palace of the Tribunat, which as there is ſome obſcurity probably owing to the conciſeneſs of the narrative, I tranſlate literally.

Two figures of the ſize of life placed at the extremity of a very extenſive apartment, hear ſpeaking with a very low voice in whatever poſition and however diſtant the perſons who ſpeak to them may be, and they anſwer all queſtions in the moſt ſatisfactory manner.

This curious experiment proves to philoſophers, that it is poſſible to aſſiſt the organ of hearing in the ſame manner as inſtruments have been contrived to aſſiſt the ſight.

The author affirms, that it is poſſible to tranſmit intire diſcourſes uttered in a low voice to very remote diſtances, without any indication of the place of the ſpeaker.

Evaporation of Water at an elevated Temperature.

Experiments on evaporation.

M. Leindenfroſt, in a diſſertation publiſhed in 1756, announced, that water loſes the quality of evaporating accordingly as the heat of the bodies upon which it falls, is augmented from the point of ebullition to the temperature of a white heat. A drop of water which he let fall into an iron ladle heated to whiteneſs, was firſt divided into ſeveral ſmall globules which afterwards united in one. Conſidering this with attention, he obſerved, that it turned with great ſwiftneſs

Experiments on evaporation.

neſs on its axis, and became ſmaller and ſmaller. After 34 or 35 ſeconds it diſappeared with noiſe. A ſecond drop which fell into the ſame ladle now ſomewhat cooled, diſappeared in nine or ten ſeconds; laſtly, a third diſappeared in three ſeconds. When the ſpherule of water in the white hot ladle was touched with a cold body, it diſappeared in an inſtant.

Klaproth has repeated theſe experiments in the following manner. He took a highly poliſhed iron ladle which he heated to whiteneſs, and threw therein a drop of water, which became divided into ſeveral globules of different ſizes, and ſoon united into a ſingle ſphere. As ſoon as this globule which exhibited the phenomena before deſcribed, had diſappeared, he let fall a ſecond, after which a third, &c. and he obſerved, that the duration of each was leſs the more the ladle was cooled. The following is the reſult of two experiments. In the firſt experiment the intenſity of the heat being greater than in the ſecond, the degree at which the water moſt ſpeedily evaporates happened later.

1ſt *Experiment.*	ſeconds.	2d *Experiment.*	ſeconds.
The firſt drop laſted	40	The firſt drop laſted - -	40
The ſecond - - -	20	The ſecond - - - -	14
The third - - -	6	The third - - - - -	2
The fourth - - -	4	The fourth - - - -	1
The fifth - - - -	2	The fifth - - - - -	0
The ſixth - - -	0		

Theſe experiments require to be made with much care. M. Klaproth obſerves, that the ſlighteſt circumſtances produce variations in the duration of the drops.

Seven drops having been ſucceſſively thrown into the ladle heated to the neceſſary degree, ſoon united into a globular maſs, which began its movement by a very rapid rotation. This maſs afterwards divided at top, when a ſpot of white froth was ſeen at the upper part of the ball, and its edges appeared indented.

This experiment was made with ten drops, and afforded the ſame reſult; but when a greater number was employed, the maſs was unable to preſerve its rotatory motion, and the whole of the water diſappeared with a hiſſing noiſe.

If inſtead of the iron ladle or ſpoon, a capſule of pure ſilver or of platina heated to whiteneſs be uſed, the appearances are nearly the ſame, but the duration of the balls is commonly longer.

A

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AND

THE ARTS.

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ARTICLE I.

An Analysis of a Variety of the Corundum. By the Reverend WM. GREGOR. *Communicated by the Author.*

THE mineral, which is the ſubject of the following obſervations, was preſented to me by my valuable and reſpected friend Philip Raſhleigh, eſq. who was ſtruck with the peculiarity of its colour. Mr. Raſhleigh informed me, that he had found its ſpecific gravity to be 3,6 at the temperature 60, and my experiments gave the ſame reſult. History of the ſpecimen of corundum; from Thibet.

The country where this ſtone was found, is Thibet: but in what particular part of that extenſive region I am not informed.

"The colour of this ſtone is motled: the prevailing tint is a diluted lilac. The maſs appears to be a confuſed cryſtallization, the parts of which are unequal, but moſtly very minute. The larger grains reflect a luſtre not inferior to that of the common adamantine ſpar. Although the adheſion of the parts of the maſs is not conſiderable, the parts themſelves have a great degree of induration, and from the ſame cauſe, probably, a greater ſpecific gravity than the maſs itſelf," which I found to be 3,603. Characters.

A.

Analyſis, pulverization, ignition, trituration.

100 grains of this ſtone, which had been reduced to a powder in a ſteel mortar, were not affected in the ſlighteſt degree by the magnet.

The powder was of a lilac inclining to a pink-colour. Expoſed to a ſtrong red heat for half an hour, it loſt $\frac{15}{16}$ of a grain in weight. Its colour was unaltered.—It was now rubbed (in a dry ſtate) to a finer powder in a mortar of flint. Upon weighing it again, I found that it had gained no acceſſion to its weight from the abraſion of it.

Boiled to dryneſs in potaſh; then waſhed; and the reſidue again treated and waſhed; and again, &c. till little remained.

It was now put into a ſilver-crucible, and covered with a ſolution of potaſh in * alkohol, mixed with an equal bulk of diſtilled water, and the crucible was placed in a ſand-bath, and the fluid gradually evaporated: and at laſt it was boiled to dryneſs. The ſalt which remained at the bottom of the crucible, was diſſolved in diſtilled water, and its ſolution poured off from a ſpungy earth, and a portion of the undecompoſed ſtone which had ſubſided. This operation was repeated in the ſame way with freſh portions of potaſh, until the whole of the ſtone was decompoſed, except a ſmall quantity of a ſpungy earth, which was thrown upon a filtre and waſhed with diſtilled water, until what paſſed through it ceaſed to cauſe the leaſt turbidneſs in a ſolution of nitrat of mercury. The edulcorating water was added to the ſolution which had been effected by potaſh.

B.

The undecompoſed reſidue was partly diſſolved by ſulphuric acid.

The powder which remained on the filtre was dried. It was of a greyiſh white colour. I put it into a ſmall matraſs and moiſtened it with diſtilled water, and then dropped ſome

* I have reaſon to ſubſcribe to the opinion of Mr. Chenevix, which he has given us in the maſterly and ſcientific paper on the arſeniats of copper, with which he has enriched the annals of mineralogy and chemical analyſis, that it is by the means of alkohol alone, that potaſh can be prepared, which is fit for delicate experiments. I was induced to employ a ſolution of potaſh in alkohol, becauſe it has been ſaid, that during the proceſs for obtaining it in a dry ſtate, it acts in ſome degree upon the ſilver veſſels, in which its ſolution is evaporated. By evaporating it *in contact with the ſpar*, I thought that this would be leſs likely to happen.

rectified

rectified ſulphuric acid upon it, and placed the veſſel in a digeſting heat. The powder became at firſt gelatinous. But this inconvenience was gradually removed, by the further addition of acid. What was diſſolved thereby was extracted by diſtilled water.

The laſt reſidue was again treated with potaſh, and its reſidue by ſulphuric acid. The ſolutions were added to the former, and a ſmall portion of ſilex remained.

The earth which remained, after having been ſufficiently edulcorated, was dried and expoſed to the action of potaſh in a ſilver crucible as before, and what the potaſh diſſolved was added to the alkaline ſolution before mentioned (A *b*). What was undiſſolved by it was heated again with ſulphuric acid, until it ceaſed to act upon it. The ſoluble part was extracted by diſtilled water. The reſiduum was thrown on a filtre and waſhed with diſtilled water till it ceaſed to affect nitrat of mercury: dried and heated red hot for half an hour, it weighed $\frac{1.5}{8}$ gr. It was pure ſilex.

The ſulph. ſolution was precip. by ammonia. It contained no lime.

The ſolution effected by ſulphuric acid, was mixed with the edulcorating water and decompoſed by ammonia. A white ſpongy earth was precipitated. When the ammonia ceaſed to cauſe any further precipitation, the clear decanted fluid was aſſayed with carbonat of ammonia and carbonat of potaſh, but no change was produced on it by the ſolution of either of thoſe ſalts. From which circumſtance the abſence of lime may be inferred.

The waſhed ammon. precip. again treated with potaſh; in part diſſolved.

The precipitate produced by ammonia was ſufficiently waſhed with diſtilled water, and in a moiſt ſtate, was boiled with the ſolution of potaſh in a ſilver crucible as before. The alkali diſſolved a portion of it. Which ſolution, together with the edulcorating water, which waſhed the remainder, was added to the alkaline ſolution before mentioned, (A *b*). What remained was thrown on a filtre, and edulcorated as before: dried and heated red hot for ten minutes, it weighed four grains.

It left oxide of titanium.

It was of the colour of pounded reſin. Expoſed to the flame of the blowpipe or charcoal it was unaltered. Moiſtened with melted tallow and ignited, it was not attracted by the magnet. It was taken up by a globule of the phoſphat of ammonia and ſoda, and ſuſpended in it in white flakes. It is ſoluble in the three mineral acids. But the ſolution, on being boiled, becomes turbid from the ſeparation of a large portion of it. It is precipitated from its ſolution in acids by

prussiat * of potash, of a beautiful grass-green, and by tincture of galls of a dark orange-colour. It possessed, in short, all the properties of the oxide of titanium.

C.

The alk. solutions sat. with mur. acid, and dissolved; precip. with carb. of potash; and edulcorated.

The alkaline solution (A *b*) was saturated with muriatic acid. A copious precipitation took place of a white earth, which was redissolved by a further addition of acid. The contents of this solution were thrown down by a solution of carbonat of potash. This precipitate collected on a filtre, was washed with distilled water, till it passed through it tasteless.

Dry precip. treated with sulph. acid left silex.

It was dried and introduced into a matrass, and rectified sulphuric acid, diluted with distilled water, dropped into it, and the fluid was evaporated. Fresh portions of acid were abstracted from it, as long as it seemed to act upon it. The soluble part was extracted by distilled water, and the residuum was edulcorated with distilled water, till it ceased to produce any change upon nitrat of mercury; dried, and heated red hot for half an hour, it weighed 10 grains. It was pure silex.

This sulph. solution with acet. of potash gave alum.

The solution which the sulphuric acid had effected, mixed with the water with which the residuum had been edulcorated, was gradually evaporated, and a sufficient quantity of acetite of potash was added to it. Regular crystals of alum were formed to the last. A small portion of silex, amounting to about $\frac{1}{2}$ a grain, was separated.

The alum. was decomp. by carb. ammon.

Distilled water was poured upon the alum, in a quantity sufficient to dissolve it in a warm state; and whilst warm, the solution was decomposed by a solution of carbonat of ammonia. The spungy earth which was separated, was collected on a filtre and edulcorated with distilled water, till it produced no effect upon nitrat of mercury.

* The prussiat of potash which I employed, was prepared by dropping a solution of tartrite of potash into a solution of prussiat of lime, as long as any tartrite of lime is precipitated. Large and beautiful crystals of prussiat of potash are produced by evaporating the fluid, which I have found to remain unaltered for several years. By this method the intrusion of the sulphuric acid is prevented: but Mr. Henry's late ingenious process seems to be, for many reasons, preferable to it. The prussiat of titanium promises I think, to be useful in the arts. Painters in oil and water colours would find in it a valuable acquisition, as a beautiful transparent green.

The

The alumina was dried gradually, and heated in a crucible, so that the bottom of it was only of a low red: it weighed 111$\frac{5}{8}$ gr. Expofed to a ftrong red heat for half an hour, it=87$\frac{3}{4}$. Sufpecting that a fmall portion of alkali was retained by it, I rubbed it to a fine powder, and digefted it with acetic acid, for many hours, and faturated it with ammonia. Precip. of earth freed from alk. by acet. acid and heat.

The alumina fufficiently edulcorated, dried and expofed to a ftrong red heat for half an hour, now weighed 81$\frac{3}{4}$. 100 grains of the original pounded fpar, from which I had taken the 100 grains on which I operated, were expofed to the fame heat, and for the fame time, and loft $\frac{15}{16}$ of a grain, as I had found before.

The proportion which the feveral ingredients of this ftone bear to each other, according to the foregoing analyfis, will be, Component parts.

Alumina	[C *d*]		81,75
	[B *b*]	1,625	
Silex	[C *b*]	10	12,125
	[C *c*]	0,50	
Oxide of titanium	[B *d*]		4
Water	[A *a*]		0,937
Lofs			1,188
			100

I have repeated the analyfis of this mineral * feveral times, and have always found it to contain the fame ingredients; though there has been a flight variation as to the relative quantities of each. This we might expect to be the cafe, not only from the lofs infeparable from the operations to which the ftone is fubjected, but alfo from the nature of the ftone itfelf, as it is not perfectly homogeneous. In one inftance, I found the alumina amount to 84 per cent. and the filex proportionably diminifhed.—Until we have better means than we have at prefent, of afcertaining the real quantities of water and oxygen which the earths and the oxides of metals feverally retain in Analyfis repeated.

* I am indebted to the kindnefs of Mr. Rafhleigh, for another fpecimen of corundum from the fame place, which differs in fome refpects in its external appearance from that which I examined. Its colour is not of fo bright a lilac, and it feems of a more compact and uniform grain. Its furface is covered with a coat of yellow mica.—This I have not fubjected to experiment.

the

the natural ſtate in which they exiſt, as ingredients of complex ſtones, we muſt conſider the reſult of chemical analyſis, but as an approximation towards the ſtrictneſs of truth.

As the intruſion of the oxide of titanium into the place, which has been uſually occupied by the oxide of iron, conſtitutes this ſtone a variety of the adamantine genus, I have been induced to record my experiments. I am aware, however, that it may be juſtly obſerved, that the oxide of neither metals is eſſential to the nature of the corundum.

Oxide of titanium often met with.

I have no doubt, but that the oxide of titanium will be found to be more plentifully ſcattered abroad throughout the mineral kingdom, than it has been hitherto imagined. I have detected it in a ſpecies of ſhirl, which occurs in a large cryſtallized maſs, on the tenement of Botallack, in the pariſh of Juſt, in this county. It compoſes ſeveral alternate beds or floors of varying dimenſions.—The compactneſs of theſe beds not having allowed the cryſtals to ſhoot with freedom in any direction; It is rarely that they occur with their terminations compleat.—Theſe, however, in no reſpect differ from the common form, and vary only in the length of the priſm, from $\frac{1}{4}$ of an inch to an inch.—The colour of this ſhirl is black.—I have alſo lately diſcovered the oxide of titanium in two ſpecies of baſalt, of the form of large pebbles, which I found near the ſea-ſhore. Whether they are natives, or whether they were dropped upon our coaſt accidentally as ballaſt from ſome veſſel, I have not as yet had an opportunity of aſcertaining. That they are *real baſalts*, I reſt upon the authority of one on whom I can ſafely rely, whoſe knowledge in mineralogy can be only equalled by his friendly diſpoſition to communicate it. I mean John Hawkins, Eſq.

My experiments alſo proved, that they contained all the known ingredients of baſalts, with the addition of the oxide of titanium.

Creed, near Grampound, Cornwall,
March 2, 1803.

Letter

II.

Letter from Mr. WILLIAM HENRY. *Concerning the Invention of Aromatic Vinegar.*

To Mr. NICHOLSON.

SIR,

I AM fenfible that an apology is neceffary for intruding on you and on your readers, a fubject which may appear, on firft view, to have little claim to general attention. If the rights and interefts of an individual were alone involved on this occafion, I fhould not have requefted a place in your Journal, for the following ftatement. But it is furely matter of general concern, that the appropriation of difcoveries and improvements fhould be dealt with ftrict juftice to their authors: for the profpect of this diftribution of "honour where it is due," is one of the moft animating principles of action; and the extinction of this motive would follow an indifference on the part of the public, to the claims of inventors.

The appropriation of difcoveries is matter of general concern.

More than fifteen years ago, during the delivery of a courfe of chemical lectures by my father, in this town, he had occafion to notice a quality of the acetic acid or radical vinegar, which had not to his knowledge been obferved before; viz. its property of diffolving camphor and various effential oils. The compound was found to poffefs a moft pungent and agreeable odour; and as the *vinaigre des quatres voleurs* had gained much reputation in preventing infection, it occurred to him that the newly difcovered folution would have ftill more powerful effects, in confequence of its high ftate of concentration. A bottle of this preparation he gave to a late active magiftrate and philanthropift (T. B. Bayley, efq. F. R. S.) who, in the courfe of an unwearied and undaunted exercife of his public functions, was frequently expofed to the dangers of foul and infected air. Mr. Bayley was highly gratified by its effects; and not only made conftant ufe of the aromatic vinegar on the bench, and on his vifits to the prifon, but introduced it to the adoption of feveral of the judges, and principal gentlemen at the bar. He alfo firft fuggefted to my father the propriety of benefiting by his difcovery, and was the medium of a connection with Mr. Bayley, perfumer, in Cockfpur Street, which has been continued to the prefent day.

Difcovery that acetic acid diffolves camphor and effential oils.

The

The sanction of an eminent physician given to a subsequent claimant.

The aromatic vinegar, like all other articles in general demand, has been a frequent subject of imitation. But it is not of this that I complain; for in consequence of unremitting attention, our preparation has maintained a decided superiority over all others, both as to quality and extent of sale. The occasion of this appeal to your readers is, that one of these imitations has been sanctioned by the name of a respectable physician, who, though not expresly yet by implication, confers on another the credit of that invention, which in justice is due to my father. *(See a letter from Dr. Trotter, physician to his majesty's fleet, contained in the advertisement of a London druggist.)*

Subsequent proceedings, &c.

From the recommendatory letter alluded to, it is sufficiently evident that Dr. Trotter was unacquainted with any prior claim to the invention of the aromatic vinegar; and he was therefore furnished by my father, in the most respectful manner, with the facts that have been laid before you. To this letter the doctor has never replied, though he declared verbally, to a medical gentleman, that my father's preparation had never fallen in his way; and that if it had, he should with equal readiness, have given testimony in its favour. The advertisement, however, still continues to be regularly inserted; and I therefore deem it expedient to appeal thus publicly, against the injustice of such a proceeding; especially in behalf of a man, who has imitated the original, only in assuming, with the coolest effrontery, an advertisement drawn up by myself.

I believe there are few of your readers, who will not decide, that the ordinary forms of civility required Dr. Trotter to have taken some notice of the letter that was addressed to him; that such an attention ought to have been paid to one of the oldest practitioners of medicine in this country; and that more respect was due to a man (whom I trust it is not unbecoming me to characterize, in terms already publicly applied to him, *viris laudatis* *) "respectable in science and literature," and "distinguished by ingenuity, honour, and the strictest integrity."

I am, Sir,

your obliged friend and servant,

WILLIAM HENRY.

Manchester, March 13, 1803.

* Dr. Aikin and Dr. Percival.

Caution

III

Caution against the Danger of leaving Phosphoric Preparations in the Vicinity of Wood. By a Correspondent.

To Mr. NICHOLSON.

March 1, 1803.

SIR,

ACCIDENTS in chemical experiments not seldom afford useful facts or suggestions; but I am doubtful whether or no the following occurrence will be thought worth notice in your Journal: however, the account of it is at your service.

Narrative of fire occasioned by oxidule of phosphorus.

While at lecture this morning, we were suddenly annoyed by a column of white fumes issuing from amongst bottles of preparations on a shelf, which was soon followed by flame. On examination I found the inflammation proceeded from a bottle containing lime, into which phosphorus had sublimed in the upper part of a tube in the making of phosphuret of lime. This was not therefore phosphuret of lime, but only the mixture of particles of ignited phosphorus, with pulverized lime scarcely ignited. In this circumstance the phosphorus I know becomes oxigenized to be in the state of *oxidule*, if not of *oxide;* which composition it is well known, I suppose, is employed for the charging of little bottles to inflame sulphur matches by mere friction of them within side of the bottle. In the present case, the bottle containing the lime with phosphorus had cracked, so as to admit air, which excited the inflammation at the temperature of about 65°. Such preparations should therefore be kept out of the way of wood, or any inflammable substance, for if this accident had happened when no one was present, the whole laboratory would have been set on fire.

Your's truly,

Z.

IV. *Description*

IV.

Defcription of a new Reflecting Quadrant. By Mr. Ezekiel Walker. *From the Author.*

To Mr. NICHOLSON.

SIR,

Obfervations on Hadley's quadrant.

THAT inftrument which goes by the name of Hadley's Quadrant, may perhaps be deemed one of the moft ufeful inventions of the laft century. Although its arc is only the one eighth part of a circle, yet it is fo conftructed as to meafure angles from 0° to 90° by the fore obfervation, and from 90° to 180° by the back obfervation. But as no method has yet been found out, by which the back horizon glafs can be adjufted with the fame exactnefs as the fore one, all angles above 90° which are taken by the back obfervation, cannot be relied on fo much as thofe that are taken by the fore obfervation. To obviate this inconvenience, the fextant was invented, by which any angle lefs than 120° may be taken by the fore obfervation. This is an invaluable inftrument to the nautical aftronomer; and indeed it is much to be regretted, that its ufe is not fo generally underftood by travellers, as the imperfect ftate of geography requires. It muft, however, be admitted, that it is not quite free from imperfections. Firft, an angle greater than 120° cannot be taken by it; and fecondly, when a large angle is taken, the rays of light fall fo obliquely upon the index glafs as may occafion fome doubt refpecting the truth of the obfervation. Mr. Ludlam gives it as a general rule, in conftructing an octant, "that very oblique reflections from the mirrors ought to be avoided *." If it be neceffary to obferve this rule in conftructing an octant, the fame fhould be attended to, as much as poffible, in taking obfervations.

Imperfections of the fextant.

New inftrument meafuring to 180° without oblique reflection.

Thefe imperfections fuggefted the idea of an inftrument to meafure any angle between 0° and 180° by the fore obfervation, without very oblique reflections from the mirrors. Plate XII. Fig. 2. exhibits a plan of this inftrument, where L M C reprefents an octant, conftructed in the ufual way, A B the

* Ludlam on the ufe of Hadley's quadrant, par. 91.

index

index glafs, *m* the horizon glafs, and H E the line of fight, or axis of the telefcope. Upon this line of fight, at an angle of 45 degrees, let another horizon glafs be fixed, reprefented by *x y*, which may be called *the fecond horizon glafs*.

When a ray of light R *n* falls upon the centre of the glafs *x y*, at an angle of 45 degrees, it will be reflected to the eye at E; becaufe the angle *x n* E (the angle of reflection) is = 45° by conftruction, and confequently R *n* H and R *n* E are right angles.

The Ufe of the Reflecting Quadrant in taking Angles.

All angles lefs than 90° are to be taken by this inftrument, in the fame manner as by the fextant. But, fuppofe it were required to obferve the fupplement of the fun's meridian altitude at fea, let the axis of vifion be directed to the zenith, and turn the quadrant till that part of the horizon, which is oppofite to the fun, be feen by reflection from the fecond horizon glafs; then turn the index from 0 towards M, till the fun appears in the telefcope, with one of its limbs in contact with the horizon, and the index will fhew the fun's obferved zenith diftance; to which add 90°, and the fum will be = the angle obferved by the quadrant, = the obferved fupplement of the fun's meridian altitude. Manner of ufing it.

To adjuft the Second Horizon Glafs.

Let the arc L N be made equal to the arc L M, and graduated from 0° at L, to 90° at N. Then, after the index error has been determined, fet the index to 90° at N, and elevate the telefcope till the fun, or fome other remote object, be feen by reflection from the index and horizon glaffes, and the fame object will alfo be feen by reflection from the fecond horizon glafs: confequently, the angle R *n* H becomes known to as great a degree of precifion as the index error. Adjuftment.

I am,

SIR,

Your obedient fervant,

EZEKIEL WALKER.

Lynn Regis, Feb. 20, 1803.

V. *Mifcellaneous*

V.

Miſcellaneous Information. Miſtake reſpecting Dr. Thomſon, Author of the Elements of Chemiſtry. Obſervations on the Scottiſh Querns. On the ſuppoſed Determination of the real Zero of Heat. In a Letter from a Correſpondent.

To Mr. NICHOLSON.

SIR,

Miſtake reſpecting Dr. Thomſon.

I BEG leave to obſerve a ſmall inaccuracy in the notice * of Dr. Thomſon's Syſtem of Chemiſtry. Thomas Thomſon, M. D. Lecturer on Chemiſtry in Edinburgh, who is much eſteemed for his extenſive knowledge, is the author of that excellent work. The Notes on Fourcroy's Elements of Chemiſtry were written by Mr. John Thomſon, ſurgeon in Edinburgh.

The following remarks are at your ſervice, from

SIR,

Your very humble ſervant,

H.

Edinburgh, Feb. 17. 1803.

1. *Obſervations on the Scottiſh Querns.*

The Scottiſh quern or hand mill.

The Indian hand mill, deſcribed in the Philoſophical Journal (III. 186), very much reſembles the hand mill or *querns* uſed by the cottagers in Scotland for grinding malt. There are many varieties of querns, but the greater number of them anſwer to the deſcription juſt quoted, to which I beg leave to refer the reader.

The querns in Aberdeenſhire have a peculiar form: a cylindrical cavity in the under ſtone contains the upper one. A plug of wood is driven in the center of this ſtone for fixing the gudgeon, and there is an eye or aperture in the angle through which the ground malt runs out. Inſtead of cutting ſo much out of the ſolid ſtone, it would be better to uſe a wooden drum. It is unneceſſary for me to give a more minute detail of the Scottiſh querns, which have been long known in this

* Phil. Jour. 8vo. III. 62.

country;

country; ſeveral pieces of them having been found in the ruins of very old buildings.

In Dumfriesſhire there is a ſmall pair of mill ſtones on a frame, driven by a crank, a vertical cog wheel, and a horizontal pinion, which grind very well, and are extremely uſeful. The ſtones have a drum and hopper like a ſingle mill. An eaſy expeditious method of reducing grain to flour, is of the utmoſt conſequence to the inhabitants of every civilized country, and ingenious men deſerve our beſt thanks for their reiterated attempts to obtain ſo deſireable an end. Mill of Dumfriesſhire.

II. *Obſervations on the ſuppoſed Determination of the real Zero.* On the zero of heat.

Many eminent chemiſts have endeavoured to diſcover the real zero, or the diviſion on the thermometric ſcale correſponding to the total privation of caloric, but their labours have hitherto been unſucceſsful. Dr. Irvine of Glaſgow contrived a theorem for this purpoſe, which he founded on ſuppoſitions and experiments; and Mr. Dalton has lately propoſed an hypotheſis for the ſolution of the ſame problem. Theſe may be conſidered in their order. Dr. Irvine. Mr. Dalton.

Without giving Dr. Irvine's theorem, which is ſo well known, I ſhall inſert the principal reſults of the experiments and calculations founded on it.

In the following tabular view, the numbers in the firſt column denote degrees of Reaumur's ſcale *above* or *below* the freezing point, and thoſe in the ſecond column indicate degrees of Fahrenheit's ſcale *above* or *below* the ſame point, accordingly as the ſign *plus* or *minus* is prefixed reſpectively. Determinations of zero from Irvine's theorem.

	Reaumur.	Fahrenheit.
M M. Lavoiſier and Laplace, from experiments on a mixture of nine parts of water and 16 parts of quick lime, place the real zero at - -	—1537°	=—3458¼°
Their experiments on a mixture of ſulphuric acid and water, in the proportion of 4 to 3 reſpectively, fix it at	—3241	=—7292¼
Their experiments on a mixture of the ſame fluids in the proportion of 4 to 5 place it at - - - - -	—1169	=—2630¼

And

	Reaumur.		Fahrenheit.
And the experiments of theſe celebrated philoſophers, on a mixture of nitrous acid and quick lime in the ratio of $9\frac{1}{3}$ to 1, give for the real zero, $-\frac{1889}{0.01783}$ - - - - -	$+10594\frac{1}{2}$	=	$+23837\frac{3}{5}$
Seguin, from the experiments of Lavoiſier on the combuſtion of phoſphorus narrated in his Elements of Chemiſtry, determines the place of the real zero to be - - - - - -	-842	=	$-1894\frac{1}{2}$
He found, by the experiments of Lavoiſier on the combuſtion of carbon, that it ſhould be - - -	-1204	=	-2709
And he inferred, from Lavoiſier's experiments on the combuſtion of hidrogen gas, that the real zero ſhould be at . - - - - -	-739	=	$-1662\frac{3}{4}$
The compariſon of the capacities of water and ice, which, according to the experiments of Kirwan, are as 1 to 0.9, fix the real zero at * - -	-600	=	-1350
Dr. Crawford, from experiments on the combuſtion of hidrogen gas, places the real zero at † - - -	$-680\frac{8}{9}$	=	-1532
Gadolin's experiments on the converſion of ſnow into water, taking the capacity of ſnow to that of water as 9 to 10 according to Magellan, place the real zero at ‡ - - - - -	$-649\frac{3}{5}$	=	$-1461\frac{3}{5}$

Theſe reſults differ widely, and one is impoſſible.

Theſe ten widely different reſults, of which one is impoſſible, induce us to conclude that one of Dr. Irvine's ſuppoſitions at leaſt is erroneous. Indeed, it ſeems to be agreed that his theorem is not well founded. The ſeventh and ninth numbers alſo ſhew, that a near approximation to accuracy is

* An intereſting Paper by M. Seguin, from which the firſt eight numbers are taken, may be ſeen in the Annales de Chimie, V. 255.

† Crawford on Animal Heat, 267.

‡ Ibid. 458.

ſcarcely

ſcarcely attainable in theſe experiments. Let us now conſider the ſecond hypotheſis.

In the Philoſophical Journal, Vol. III. page 130, there is a very important paper of Mr. Dalton's on the Expanſion of Elaſtic Fluids, where he ſays, " In order to explain the manner in which elaſtic fluids expand by heat, let us aſſume an hypotheſis, that the repulſive force of each particle is exactly proportional to the whole quantity of heat combined with it, or in other words, to its temperature reckoned from the point of total privation: then, ſince the diameter of each particle's ſphere of influence is as the cube root of the ſpace occupied by the maſs, we ſhall have $\sqrt[3]{1000} : \sqrt[3]{1325}$ (10 : 11 nearly) : : the abſolute quantity of heat in air of 55° : the abſolute quantity in air of 212°. This gives the point of total privation of heat, or the abſolute cold, at 1547° below the point at which water freezes."

Mr. Dalton's hypotheſis.

According to this hypotheſis, I have computed the following numbers, where the ſame is to be underſtood as in the laſt table, except the degrees in the firſt column, which are thoſe of the centigrade thermometer.

Computations of zero made from the ſame.

	Celſius.	Fahrenheit.
$\sqrt[3]{1000} : \sqrt[3]{1325}$: : 10 : 10.983446,		
which places the real zero at -	—874°.12=	—1573°.43
$\sqrt[3]{1000} : \sqrt[3]{1375}$: : 10 : 11.1199,		
which fixes it at - - -	—892.93=	—1607.29
$\sqrt[3]{1167} : \sqrt[3]{1325}$: : 10.528 : 10.9834,		
nearly, which gives - - -	—952.34=	—1714.2
$\sqrt[3]{1000} : \sqrt[3]{1167}$: : 10 : 10.528268,		
and the real zero is at - -	—827.77=	—1485.98

Theſe deductions do not differ nearly ſo much as thoſe in the firſt table; yet they give grounds for believing that the ſuppoſition, or the greater part of the experiments is erroneous. It is no ſupport to the validity of this hypotheſis to ſay, that the firſt number nearly agrees with one deduced from a method totally different; for it might have been conſiderably greater or leſs than it is, and yet have been nearly equal to one of thoſe in the firſt table. There ſeems to be a ſlip in the degrees mentioned in Mr. Dalton's paper; for 167 + 158 = 325, the whole dilatation between 55° and 212°, while $77\frac{1}{2}$°

Theſe reſults apparently of little value.

 + $77\frac{1}{2}$°

$+ 77\frac{1}{2}° = 155°$ only, which ſhould be 157°, of which the half is $78\frac{1}{2}°$. This might eaſily happen: in the calculations of the third and fourth number I have uſed $78\frac{1}{2}°$.

Mr. Dalton and Cit. Gay Luſſac have conſiderably improved the method of experimenting on gaſes, and it is to be hoped they will puſh their improvements ſtill further.

II.

VI.

Outline of the Craniognomic Syſtem of Dr. Gall of Vienna. By Dr. BOJAMES.

(Concluded from Page 202.)

5. *Organ of the Inſtinct of Sexual Union,*

5. Sexual union. THE organ of the inſtinct of ſexual union is ſituated at the baſe of the *occipital*, behind the *medulla oblongata*, and the large aperture of the *occipital*.

This organ is not developed until the age of puberty; and it in a great meaſure influences the figure of the back of the neck, becauſe the muſcles are attached to this part of the *cranium*.

The developement of this organ does not take place in animals which are caſtrated before puberty, for which reaſon it is invariably obſerved that bulls have much ſtronger necks than oxen, and " that horſes which have undergone this operation, before the neck is formed, are always ſlender in that part."

In the monkey, the hare and the cock, this organ is very diſtinct, and in pigeons and ſparrows the *occiput* forms a peculiar ſack, which appears to be an appendage to the head; it is alſo known that theſe animals have a ſtrong diſpoſition to copulate. The ſame configuration is ſometimes found in the cranium of men, and Gall poſſeſſes in his cabinet ſeveral ſkulls of idiots, who were noted for laſciviouſneſs, in which the *occiput* has a very unuſual projection.

6. *Organ of the Mutual Love of Parents and Children.*

6. Parental and filial affection. The organ of the mutual love of parents and children occupies all the back and upper part of the *occipital;* from its poſition

tion it is intimately connected with the preceding organ, which consequently influences this by its action. "The excessive developement of this organ sometimes contributes to form that prolongation of the *occiput* of which we spoke in the preceding article."

This organ is in general more distinct in women than in men, and throughout nature it is more defined in the female than the male; it is more particularly apparent in monkeys, whose fondness for their young is so remarkable as to have become proverbial.

"In general, all those animals which shew much affection for their young are provided with it; and it appears to us that pigeons, the male of which sits on the eggs as well as the female, and which feed their young nestlings by a kind of rumination, may be taken as an example."

The cuckow, which never rears its young, is almost entirely destitute of this organ.

7. *Organ of Attachment, of Friendship.*

Behind and between the parietals, and on the lateral parts of the *occipital,* is placed the organ of attachment, or of friendship. 7. Attachment; friendship.

"By its position it has an intimate connection with the two preceding organs, and it is in animals destined to live in societies that the united action of these three organs takes place."

Dogs shew the most astonishing marks of attachment, and spaniels, terriers, and house-dogs afford the greatest number of examples; these species are also distinguished by a large head, in which the developement of this organ is found behind and above the *zygomatic apophyses.* The head of the greyhound, which is less susceptible of attachment, is narrower behind, and usually without this organ.

8. *Organ of Courage.*

The organ of courage is placed at the posterior and inferior angle of the parietals. It assists in increasing the size of the head and separating the ears from each other. Its proximity to the three preceding organs accounts for the fury of animals in rutting time, and for the extraordinary courage of those which have young, or which protect their females or the individuals of their society. 8. Courage.

It is moſt diſtinct in the hyena, the lion, the wolf, ſome ſpecies of dogs, and particularly in the wild boar, the fierceneſs of which is well known.

On the contrary, the aſs, the greyhound, the ſheep, the hare, which are diſtinguiſhed by their timidity, are deſtitute of this organ; their heads are narrow behind, and their ears almoſt cloſe together.

A ſingular phenomenon ſeems to give countenance to the opinion of Gall reſpecting the ſituation of this organ; it is the involuntary action of a man who loſes his courage. He rubs behind his ears, as if endeavouring to ſtimulate the action of that organ from which this faculty is derived.

Note, "We have noticed an action of cats which appears to have ſome reſemblance to the above, and which regards the organ of attachment. It is, that in careſſing man they rub againſt him with the back part of the head."

9. *Organ of the Inſtinct to Aſſaſſinate.*

9. Aſſaſſination. The organ of the inſtinct to aſſaſſination is ſituated more forward than the organ of courage, towards the middle of the ſides of the parietals.

It is developed in all the carnivorous animals who live by prey; and Gall found it in the ſkulls of ſeveral murderers.

10. *Unknown Organs.*

10. Unknown. Two organs which correſpond with the temporal bones, are yet unknown as to their functions.

11. *Organ of Cunning.*

11. Cunning. The organ of cunning occupies the front and lower parts of the parietals; it is developed in all thoſe animals which are diſtinguiſhed for this faculty, ſuch are the fox, the pole-cat, the cat, the diver *, and it is in the moſt intimate combination with the *organ of theft*, which is only a prolongation of this towards the orbit, and is found in cats, ſome dogs, and in magpies.

* An obſervation which it appears difficult to reconcile to this, is that Gall conſtantly obſerved this organ to be developed in poets; he gives no explanation, but his obſervation is accurate.

It

It is perhaps to the developement of this organ that the Calmucks, whofe national character is that of theft, are indebted for that magnitude of their heads which has been noticed by fome obfervers.

12. *Organ of Circumfpection,*

The organ of circumfpection is fituated in the middle of the parietals, above the organ of cunning, and that of the inftinct to affaffination. 12. Circumfpection.

The exceffive developement of this produces irrefolution, and its defect caufes heedleffnefs; it is diftinct in the chamois and the rein-deer, the circumfpection of which is remarkable, and which do not tread over unknown paths but with the greateft precaution.

It is alfo found in fuch animals as only leave their retreats at night, fuch as owls, otters, &c.

13. *Organ of the Inftinct for Self-Elevation.*

This organ, in the middle of the inner border of the parietals, a little farther back than the middle of the upper part of the head, gives us a true notion of the difficulties to be overcome in the refearches of Gall, and at the fame time affords a ftriking example of the happy opinions of this accurate obferver. 13. Self-elevation.

He found this organ well developed in the chamois, and ftill more fo in the wild goat; he alfo noticed the fame in many men diftinguifhed by their pride. It was difficult to bring thefe obfervations into one point of view; but on confidering that the chamois frequents the moft lofty parts of mountains, that the wild goat is conftantly endeavouring to afcend higher, and that pride, attentively examined, is only the defire to be fuperior to others, he was perfuaded that this was the organ which produced thefe effects, *apparently fo differing,* and he took it for the organ of the inftinct of raifing or felf-elevation.

The head of the proud man, raifed and thrown back, tends to confirm this opinion ftill more.

Note, "It appears to us that the figure of a proud man, oppofed to that of a fubmiffive and modeft man, renders the truth of this notion more obvious. In the firft every thing is directed upwards; he fets up his hair, raifes his head, lifts

his brows, turns up his eyes, throws back his ſhoulders, walks on tip-toe, and conſiders every ſurrounding object as beneath him; in the other, on the contrary, the hair falls naturally, the eyes, the eye-lids and the head are turned downwards, the body and the knees are ſlightly bent; in ſhort, every thing indicates a ſtate of ſubmiſſion, without a wiſh for ſuperiority."

14. *Organ of the Love of Glory.*

14. Love of glory.

When this organ is extended farther on the ſides, it forms that of the love of glory, a propenſity very analogous to pride.

15. *Organ of the Love of Truth.*

15. Love of truth.

The function of the organ which is ſeen at the poſterior and ſuperior angle of the parietals, is not exactly fixed by Gall; nevertheleſs he has reaſons to conſider this angle as the ſeat of the organ of the love of truth; but he has not yet collected a ſufficient number of facts to produce entire conviction.

Note, "We have ſome difficulty to perſuade ourſelves of the function attributed by Gall to this laſt organ; it appears to us, that an organ found among thoſe with which animals are provided as well as men, ought not to be appropriated to a faculty, like the attribute of veracity, is adapted only to the latter.

"Nevertheleſs this faculty, like that of pride, may be capable of great modifications in animals: and we acknowledge that we have known two men, one of whom was diſtinguiſhed by extreme veracity, and was furniſhed with this organ in a very eminent degree; but the other, on the contrary, whoſe diſpoſition for falſhood was extraordinary, was ſo entirely deſtitute of it, that inſtead of a projection, there was a cavity in this part of the head."

In the anterior, or lower part of the *os frontis,* Gall diſcovered many organs, the functions of which are of great importance.

At the commencement of his inveſtigations, he conſidered them as organs of the different ſpecies of memory; but afterwards obſerving that their action was not merely reproductive, but alſo productive, he was induced to conſider them as the organs of particular ſenſes, and to eſtabliſh, from this obſervation, the opinion that memory in general is only the reproductive operation of all the organs; and that imagination, on the contrary, is their productive action.

The

The automatic motion of man who endeavours to recollect, feems to relate to thefe organs. He places his hand, unconfcioufly, upon the lower part of his forehead. This action, though unobferved by him who performs it, is neverthelefs conftant, and is never confounded with that mentioned before, under the head of the organ of courage.

16. *Organ of the Senfe of Locality.*

The organ of the fenfe of locality occupies the fore part of the *os frontis*, which correfponds to the protuberances above the orbits *(protuberantiæ fuprà orbitales)*; it is generally found in the craniums of thofe who are diftinguifhed by large frontal finufes, and uniformly have an anterior cavity adapted to an elevation of the brain. 16. Senfe of locality.

When it acts reproductively, it conftitutes what we term local memory *(memoria localis)*; by its productive operation, on the contrary, it inclines to combinations of new localities.

It is this organ which, in unknown places, directs the blood-hound, in whom it is ftrongly defined; it exifts in all birds of paffage; it incites them to change of place, to take long flights, and to return to their firft habitations: the ftork and the fwallow are eminently provided with it, as well as thofe animals which remove the fartheft from our climates. In fuch men as are furnifhed with it, we alfo difcover a ftrong remembrance of places and a defire to travel; it is alfo conftantly found in good painters of landfcapes.

" A general, who regulates the difpofitions of an army, and who at a glance muft difcern all the localities of the country he occupies, cannot fucceed without it." The great Frederic furnifhes us with a ftriking example. In advanced age, this organ is one of thofe which fenfibly diminifhes: it is alfo notorious that every kind of memory and of imagination grow weaker as the individual becomes older; the frontal finufes then increafe within; the action of the brain is no longer capable of obftructing their developement.

17. *Organ of the Senfe of Things* (fenfum rerum.)

The fenfe of things has its correfpondent organ in the lower and anterior part of the *os frontis*, between and higher than the preceding; its action is both productive and reproductive; and, in the latter cafe, it gives the remembrance of facts and things. 17. Senfe of things

This

This organ is very neceſſary to education and inſtruction, which abſolutely require the recollection of paſt circumſtances; in age it is ſubject to the ſame change as the preceding.

Among animals, the elephant is particularly diſtinguiſhed by the developement of this organ; this animal alſo retains with the greateſt accuracy thoſe circumſtances and acts which have a reference to itſelf.

" Among men, we have found this organ, not only in thoſe whoſe memory of facts and objects was powerful, but alſo in thoſe which might be denominated ſyſtematic heads, from arranging facts in order, and hence forming concluſions; in thoſe who poſſeſs a happy conception, and are diſtinguiſhed by a deſire of univerſal knowledge; it even appears to us, that the operation of combining facts with a view to obtain a reſult, is one of the principal actions of this organ; at leaſt the elephant, who keeps his trunk filled with water, for the purpoſe of throwing it over the man who had offended him the night before, arranges a number of facts, and draws from them a reſult which is a true logical concluſion; and we are not acquainted with any other organ in the elephant to which this action can be referred.

" The automatic movement of the man who perceives that he has reaſoned wrong ſeems to give effect to theſe conjectures. He ſtrikes himſelf on the middle of the forehead."

18. *Organ of Painting, Senſe of Colours.*

18. Painting or colours.

The organ of the ſenſe of colours, or of painting, occupies the fore part of the *os frontis*, above the orbit; Gall has obſerved this organ in all painters of eminent talent.

" Having been acquainted with this diſcovery but a ſhort time, we have not been able to collect many obſervations on it; we have neverthelefs remarked it in ſome individuals, and it is very apparent in the head of Raphael, in the National Muſeum, No. 57."

19. *Organ of the Senſe of Numbers.*

19. Senſe of numbers.

The organ which correſponds to the lower and exterior part of the *os frontis*, near the zygomatic apophyſis of that bone, poſſeſſes the function of the ſenſe of numbers; it exiſts in men whoſe memory is good with reſpect to numbers, and in arithmeticians who combine calculations with facility; it exiſts

exifts in a fpecies of magpie capable of counting as far as nine, which is the only example known among animals.

"We have had an opportunity of noticing this organ on the head of a blind man, at the Quinze-Vingts, remarkable for his arithmetical talents; and Gall preferves the bufts of many men which afford very inftructive examples."

20. *Organ of the Mufical Senfe.*

Above this organ is found that of mufical fenfe, or for founds. 20. Mufic.

It acts in a manner fimilar to the other organs, productively and reproductively; it gives the memory for founds; it facilitates the new combinations of mufical compofition; it induces birds to fing; it acts upon thofe who learn to fpeak, and in whom language is founded only upon this remembrance of founds.

It is intirely wanting in animals which have no mufical fenfe; it is ftrongly developed in the parrot and the ftarling; and thofe great muficians Gluck, Mozart, Haydn, Pleyel, furnifh us with ftriking examples.

21. *Organ of Senfe for Mechanics.*

In the lateral and inferior part of the *os frontis* is found the organ of fenfe for mechanics. The beaver which forms its habitation is eminently provided with it; it exifts in the field-moufe and in the birds which make their nefts with much art; it is met with in men of mechanical talents, who conftruct with eafe any machine, who ufe their hands with dexterity, and who excel in the different arts which require manual labour. Though it may be difficult to judge of the exiftence of this organ, when it is but flightly developed, "becaufe the temporo-maxillary mufcle covers this part of the cranium; it is neverthelefs very apparent if the faculty exift in a fuperior degree, and it is then one of thofe organs refpecting which there can be the leaft doubt. 21. Mechanics.

22. *Organ of Verbal Memory.*

In the interior of the orbit, at the bottom of the upper part, exifts the organ of verbal memory; it may be noticed from its developement by the effect it produces on the pofition of the ball of the eye, which it impels forwards, and more or lefs out of the orbit. 22. Verbal memory.

Perfons

Perſons provided with it eaſily retain words by heart. Gall, while young, remarked this faculty in ſeveral of his ſchool-fellows, who excelled merely by this talent, and who were diſtinguiſhed by the protuberance of their eyes. This was the firſt obſervation which afterwards led him to theſe inveſtigations. A number of ſubſequent obſervations have eſtabliſhed the truth of its exiſtence and of its function.

23. *Organ of Senſe for Languages.*

23. Languages. The organ at the exterior and upper part of the orbit, is called by Gall, the organ of ſenſe for languages. Its preſence has a conſiderable influence upon the poſition of the ball of the eye; it puſhes it downwards and towards the noſe, and increaſes its diſtance from the upper edge of the orbit; it never exiſts in animals, in whom the ball of the eye is directed more towards the exterior ſide of the orbit.

Diſtinguiſhed talents for the languages are invariably attended by its developement; it is eminent in great philologiſts; and though it may be difficult to decide from external appearances, reſpecting its exiſtence, we have nevertheleſs remarked that it never eſcaped the penetrating eye of Gall, and that he was never miſtaken in this point.

24. *Organ of the Memory of Perſons.*

24. Memory of perſons. The function of the organ at the upper internal part of the orbit, has not yet been diſcovered by Gall; nevertheleſs many obſervations on man and animals, ſuch as the dog and the horſe, induced him to ſuppoſe it the organ of recollection of perſons. The developement of it, like that of the preceding, muſt influence the poſition of the eye; it ſhould contribute to remove it from the upper edge of the orbit, and to puſh it towards the external ſide, if an equal developement of the preceding organ do not counterbalance its action.

25. *Organ of Liberality.*

25. Liberality. The organ of liberality is ſituated in the anterior part of the *os frontis*, above that of the ſenſe of locality, and of the ſenſe for painting (Nos. 16 and 18), and beſide that of the muſical ſenſe (No. 20); its extreme developement is a concomitant of prodigality; it it not to be found in the miſer; in that caſe, a hollow is formed in this part of the *os frontis*. Gall is in poſſeſſion of numerous examples.

" The

" The proximity of the organ of mufic, and that of the fenfe for painting (Nos. 18 and 21) appear often to affift the developement of that of liberality; this is perhaps one reafon why we fo often find prodigality among men of eminent abilities in thefe fciences."

We conftantly obferve, that as a man becomes old, he becomes covetous; thus in advanced age the diminution of this organ is fo remarkable, that its place is frequently occupied by a cavity in the *os frontis*, which is fometimes of confiderable fize.

26. *Organ of the Power of comparing Things.*

The organ above the fenfe of things, in the middle of the forehead, is deftined to a faculty which Gall calls the fpirit of comparifon *(judicium comparativum)*. 26. Comparing or combining.

It forms an oblong eminence, and is found in men who avail themfelves eafily of figures and images in converfation; who are not at a lofs for expreffions; who narrate fluently; and poffefs great eloquence.

27. *Organ of Metaphyfical Talent.*

When this organ is more developed towards the fides, fo as to form a round prominence, raifed in the middle of the forehead, it is the index of metaphyfical talent. Among the bufts of ancient philofophers, that of Socrates affords the moft ftriking example; among modern philofophers remarked for this organ, I fhall only notice Kant as one of the moft celebrated. 27. Metaphyfics.

Note, " I recollect in one of my firft fchool-fellows, to whom we had given the name of philofopher on account of his attachment to the abftract fciences; that his forehead prefented a very vifible developement of this organ."

28. *Organ of the Talent for Obfervation.*

The organ of the talent for obfervation fpreads over the whole of the anterior part of the *os frontis*, and its developement approaches more or lefs to the front of the vertical line. It is found on the craniums of all the men of obfervation in all ages; the celebrated phyfician Frank poffeffes it in an eminent degree, and Gall himfelf is very evidently furnifhed with it. 28. Obfervation.

29. Organ

29. *Organ of the Talent for Satire.*

29. Satire and wit.

The organ of ſatire and facetiouſneſs (*witz* of the Germans, *wit* of the Engliſh, *facetiae* of the Latins) correſponds with the frontal protuberance. Gall preſerves many examples of the truth of this opinion, and we have uniformly found it true.

30. *Organ of Goodneſs.*

30. Goodneſs.

The organ of goodneſs is in the middle of the forehead, above that of compariſon (No. 26.) It forms that oblong elevation which is conſtantly found in the portraits of Chriſt and of Mary, painted by Raphael and Correggio, and contributes much to that expreſſion of mildneſs and goodneſs with which we are delighted; it is always ſeen in the craniums of men naturally good, and is wanting in thoſe of the miſchievous and vindictive *.

Among animals the roe-buck, the hind, the pigeon, &c. are provided with it; on the contrary, animals of prey, ſuch as the eagle, the ſtarling, the tiger, the fox, &c. are without it; in the latter caſe, the *os frontis*, inſtead of being round and elevated, is depreſſed and hollow.

31. *Organ of Muſic or of Theatrical Talent.*

31. Theatrical repreſentation.

The very marked enlargement of the ſummit of the *os frontis*, is owing to the developement of the organ for the repreſentation of thoughts by actions, the organ of muſic, or theatrical talent.

" Gall has collected many obſervations to prove the truth of this opinion, nor will it be overlooked inattentively, conſidering the heads of the great performers at the different theatres of Paris."

Note, " We think we have alſo obſerved that this organ is particularly developed in the deaf and dumb, and we attribute it to the neceſſity ſuch perſons are under of acting continually, an exerciſe which neceſſarily facilitates its advancement."

* We are not here ſpeaking of that goodneſs which is the reſult of moral principles, that to which we allude exiſts as inſtinct, without being the produce of moral reflections.

32. *Organ*

32. *Organ of Theoſophy.*

The organ of theoſophy occupies the moſt elevated part of the *os frontis*. 32. Theoſophy.

All the portraits of ſaints which have been preſerved from former ages, afford very inſtructive examples, and if this character is wanting in any one of them, it will certainly be deſtitute of expreſſion.

It is *exceſſively* developed in religious fanatics, and in men who have become recluſe through ſuperſtition and religious notions.

It is the ſeat of this organ, which according to Gall, has determined men to conſider their gods as above them in a more elevated part of the heavens. In fact, when we conſider this ſubject philoſophically, there is no more reaſon for ſuppoſing that God is placed above the world, than there is to ſuppoſe him below it.

33. *Organ of Perſeverance.*

The laſt of the organs hitherto diſcovered by Gall, is that of perſeverance, of conſtancy, of character; it is ſituated in the anterior and ſuperior part of the parietals in the middle of the head; when it is in exceſs it cauſes obſtinacy, but its defect produces inconſtancy. 33. Perſeverance.

" With regard to thoſe parts of the ſkull in which Gall has not yet diſcovered organs, it is probable that his future inveſtigations will afford him the means of ſucceſs; on this ſubject, the work which he is about to publiſh, will give us more ample details. It will alſo be for him to convince us, by arguments perhaps inconteſtable, of the truth of his ſyſtem, the expoſition of which cannot be very ſatisfactory in a curſory outline."

We think it neceſſary alſo to remark, that the organs here enumerated are not diſtinctly perceptible, except in individuals who poſſeſs ſome faculty in an eminent degree, and that it is not poſſible to form a correct judgment of a moderate talent, on account of its organ being confounded among thoſe which ſurround it. " We ſee no reaſon, philoſophically ſpeaking, for the calumnies which have been lately thrown upon the ſyſtem of Gall, that *it tends directly to materialiſm.* When we admit of organs of the action of the internal faculties, the immeaſurable ſpace between mind and matter will continue

continue the fame. Objects of a nature fo unlike, are incapable of union. On the other hand, the will of man continues unimpaired; it is this which muft counterbalance the operation of the organs, morality ought to fubdue the paffions."

BOJAMES, M. D.

VII.

Method of conveying Boats or Barges from a higher to a lower Level, and the contrary, on Canals, by means of a Plunger, inftead of lofing Water by Locks. By LAWSON HUDDLESTON, *Efq. of Shaftfbury, Dorfet. Communicated by the Inventor.*

Conftruction of a lock for raifing and lowering boats.

WHEREVER there is occafion to convey a commercial boat or barge from a higher canal to a lower (ufually termed a higher or lower line) or *vice verfâ*, a lock muft be conftructed of ftone or other fit materials in the fpace between the higher and lower canal, fo as to communicate at the ends with both of them. The dimenfions of the lock are to be as follow; its horizontal fuperficies or area fhould correfpond both in form and fize with that of the boat itfelf, with the allowance of fufficient room only for the boat to rife and fink freely within it; and its depth and height fhould be fuch that the water within it may rife with a loaded boat floating therein from the level of the lower to that of the higher canal: there muft alfo be two fluices one at each end of the lock, large enough to admit the free ingrefs and egrefs of the boat, one of which muft be accommodated to the level of the higher canal, the other to that of the lower.

Refervoir on one fide of the lock,

On one fide of the lock there muft be a refervoir of equal height with that of the lock, the form of which may be rectangular or not (though the former may perhaps be the more convenient) the area of which fhould be equal to, or rather in practice fomewhat exceeding, that of the lock. In *this* cafe the perpendicular depth of the refervoir fhould be equal to double the intended rife or fall of the veffel within the lock; but where the areas of the lock and refervoir are *unequal* this proportion varies: for as in all cafes the quantity of water to

4 be

be difplaced out of the refervoir into the lock muft be equal in cubical content to that of the fpace within the lock between the furface of the lower line and that of the upper, it follows that if the area of the refervoir be greater than that of the lock the neceffary depth of the water to be difplaced will be fo much the lefs than in the cafe before ftated; and *e contra* if the area of the refervoir be lefs than that of the lock the neceffary depth of the water to be difplaced muft be fo much the greater: fo that whether this proportional quantity of water is to be obtained by the greater depth or the greater area of the refervoir, is a point for the confideration of the artift.

There muft be a communication fomewhere near the bottom between the lock and refervoir, that the water in each may be always on a level; which level may by means of the lower fluice always correfpond with that of the water in the lower canal, except during the actual operation of raifing or finking a boat within the lock.

and communicating with it,

To the infide of the refervoir muft be fitted a * folid body or plunger, in fpecific gravity fomewhat exceeding water, and of fuch a bulk and form as will exactly fill the whole of the refervoir, allowing only fufficient room for the plunger to move freely up and down therein.

in which a plunger may be raifed or depreffed.

Let us now fuppofe the refervoir to be 16 feet deep, and by means of the communication with the lower canal to be precifely *half* full of water. Let us alfo fuppofe the plunger to be fo fufpended by machinery as to be movable up and down within the refervoir, and to be barely above the furface of the water, when it, [the plunger] is at its greateft height. And now let us fuppofe that a boat or barge is floated through the lower fluice into the lock. That fluice being now fhut, as well as the upper one (which was all along fuppofed to be fo) the plunger is let down to the bottom of the refervoir: by this operation though the plunger be not in actual contact with the fides of the refervoir, much lefs what is called *water-tight*, (a neceffary circumftance in the cafe of forcing-pumps, piftons, &c.) the water in the refervoir will be forced into the lock, and thereby raife the boat or barge as much as the plunger finks, viz. 8 feet: in which fituation the water in the lock

Method of operation: as the plunger fubfides the boat rifes and the contrary.

* At leaft fo far folid as to outweigh its bulk in water.

lock being now on a level with that in the upper canal, the upper ſluice may be raiſed, and the boat floated out. The plunger being ſtill *down*, if a boat be floated into the lock from the upper canal, let the upper ſluice be ſhut and the plunger be raiſed, and the boat will in like manner deſcend in the lock to a level with the lower canal.

Hydroſtatic principle.

It is obvious that the *principle* of this invention is founded on this law in hydroſtatics, viz. that two columns of water however different in lateral dimenſions, will, if there be a communication between them, always maintain one and the ſame level.

The plunger is counterpoiſed.

The plunger is to be counterpoiſed by a weight acting on a ſpiral curve (on the ſame ſhaft with the wheels or mechaniſm that raiſe the plunger) in order to accommodate the action of the counterpoiſe to the *decreaſing* weight of the plunger as it deſcends into the reſervoir; ſo that being *counterpoiſed* it may be eaſily raiſed or ſunk by means of the mechanical apparatus uſually employed for ſuch purpoſes, poſſibly indeed this movement may alſo be effected by means of a fire-engine, but the *expediency* of ſuch means is not meant to be ſuggeſted.

General remarks.

In theory the wall or partition common to both the lock and reſervoir may not be neceſſary, however uſeful it may be in practice.

Should occaſion require it, the reſervoir may be placed at any given diſtance from the lock, provided there be a communication between them, at or near the bottom.

Without ſuggeſting any preciſe limitation (which can be learned only from experience) the projector conceives that locks on this conſtruction are better adapted to low or moderate lifts than to high ones; boats alſo of a moderate ſize (as recommended by Dr. Anderſon and Mr. Fulton) and ſo conſtructed as to exceed rather in *depth* than in length or breadth, ſeem beſt adapted to the ſcheme now propoſed.

Advantages of this lock.

On a compariſon then between this lock and others of a late invention, it is preſumed that wherever it may be expedient to uſe it, it will be attended with the following material advantages.

No machinery except to the plunger;

1ſt. The only machinery neceſſary (the two ſluices excepted) in this lock, is for the purpoſe of raiſing and ſinking the plunger or plungers.

2d. Whilſt

2d. Whilſt the machinery in other contrivances above alluded to, is either wholly or in part liable to injury from water, the machinery in this (the two ſluices as before excepted) is wholly exempt from this diſadvantage. not liable to injury from the water;

3d. Whilſt caiſſoons, coffers, &c. require not only the opening of one or more ſluices, but alſo a previous fitting or adjuſting of their ends to the mouth of the canal to prevent the loſs of water, before the boat can enter the lock; according to this plan, on the opening of a ſingle ſluice the boat floats at once into the Lock without loſs of water, and without any ſuch delay or difficulty: when I ſay without loſs of water, it muſt be underſtood with this allowance, that if a boat goes down laden, and returns (or another goes up) unladen, the upper level gains a body of water equal to the weight of the cargo, *e contra* the lower canal gains ſimilarly if a boat goes down unladen and returns laden. nor requiring cloſe fittings. No loſs of water.

4th. If the water in the upper canal at any time be ever ſo abundant, no boat can paſs through the former locks but *by working the machinery;* whereas according to this plan the *machinery may be at reſt,* and the boats paſs on as through the common gate locks, whenever the upper canal can afford the neceſſary expence of water. If water be abundant, the machinery need not be uſed,

Laſtly. If at any time the machinery of any *one* lock of the former kind (out of the many that may be neceſſary in a given length of canal) ſhould happen to be diſordered, the *whole navigation is at a ſtand,* whereas in a lock of this conſtruction in caſe of ſuch an accident, the boats may ſtill paſs and repaſs as through a common gate lock. and therefore the navigation will not be impeded by any inevitable diſorder.

Let us ſuppoſe a boat (or barge) to be 20 feet long, and ſix feet wide, and to draw three feet water. The ſolid content will be 360 cubic feet, each of which weighing about 62 pounds, the burthen will be nearly 10 tons. Particular ſtatement of dimenſions, &c.

Is it propoſed to raiſe (or ſink) this boat eight feet, by means of a plunger of equal area or horizontal ſuperficies with that of the boat.

The deſcent of the plunger muſt in *this* caſe be equal to the aſcent of the boat, viz. eight feet, and the height of the plunger muſt (as in *all* caſes) be equal to the ſum of both, viz. 16 feet; the ſolid content of the plunger therefore will be 1,920 cubic feet, amounting in weight to nearly 53 tons.

Let

Mechanic force faved by enlarging the horizontal fection of the plunger.

Let us now fuppofe that *two* fuch plungers are joined together laterally, fo as to form a new plunger, of the fame height, but of twice the area and weight of the former. It is plain that this new plunger (moving in a refervoir adapted to its dimenfions) will by defcending four feet, raife the boat in the lock as much as the other by defcending eight feet, viz. eight feet:—and the momenta (confifting of weight multiplied by velocity) of both thefe plungers will be equal, becaufe the product of 53 [tons] multiplied by eight [the velocity] equals the product of 106 [tons] multiplied by four [the velocity] each amounting to 424.

But it will be found that, as in *all* cafes the height of the plunger is equal to the fum of its own defcent added to that of the boat's afcent, the height of this new plunger, inftead of amounting to 16 feet, will only amount to 12: its weight therefore will be only $79\frac{1}{2}$ tons, and its momentum 318: it will therefore require fo much the lefs time or labour to work it, in the ratio of 318 to 424, or three to four. Qu. Would not this objection arifing from the neceffity of having ftronger machinery to fupport this latter plunger, be counterbalanced by the advantage of working it with fo great a faving of labour and time?

Remedy for depofition of fediment.

Should it be apprehended that, notwithftanding the *forcing* effect of the plunger, the water in the refervoir may gradually depofit fuch a quantity of fediment at the bottom as to obftruct the defcent of the plunger, it is obvious, that there may be a depending drain under the refervoir to be opened occafionally, or the bottom of the refervoir itfelf may be fo much below the loweft point of the plunger's defcent, as to render the cleanfing it very feldom neceffary *.

The machinery may be worked by wafte water.

It remains to be obferved, that as every canal ought to be fo well provided with water as to allow of *fome* wafte (though not enough perhaps to fupply the lofs of a lock-fall with every boat that paffes) the machinery of the lock may be worked thereby, without the mechanical aid commonly employed for fuch purpofes; for if the plunger preponderates over the refifting medium, and counterpoife barely enough to overcome

* The impetus with which the water could rufh out of the refervoir into the lock, by the action of the plunger, could probably raife and force out any fediment, except ftones or gravel.

the

the friction of the axis, &c. it will at all times descend slowly within the reservoir, and raise the boat, when left at liberty so to do; and if instead of being allowed to descend the whole eight feet required, it be stopt after having descended seven feet, the admission of one foot depth of water from the upper line into the lock, may be sufficient to raise the plunger again with the loss of only $\frac{1}{8}$th part of a lock-fall of water.

Plate XII. Fig. 1. Exhibits a perspective view of a model of the apparatus above described, having part of the front taken away in order to shew the interior parts. View of the apparatus.

A. Represents the upper sluice of the lock.

B. The lower sluice.

C. The plunger.

D. The snail upon which the weight or chain is wound that carries the counterpoise.

VIII.

An easy Method of Churning Butter. By CIT. JUMILHAC, *President of the Society of Agriculture, of the Seine and the Oise**.

TO increase the powers of man, is one of the attributes of mechanics, but in order that a mechanical contrivance may be truly useful, it is requisite that it it should be simple and cheap. That which I now offer to the society, appears to me to unite these two advantages.

It is universally known, that in great heats and extremely cold weather it is difficult to churn butter. The labour of several persons successively, is often applied without any success, to procure the perfect coagulation of the cream. In vain during the rigour of winter, the cream is placed by the fire, or mixed with milk quite fresh and yet warm; and it is with as little success, that in summer the churn staff is occasionally immersed in cold water. All these means though of value, are insufficient if not seconded by celerity of motion in the act of churning.

Surprised at the slowness with which butter is formed, especially in winter, and after having continued the operation for

* Sonini's Bibliotheque Phys. Oeconomique, No. 1.

 five

five hours and a half, I thought I ſaw that the difficulty aroſe from the aukwardneſs and conſtraint of the arm that holds the churn ſtaff; both which are increaſed in proportion as the cream becomes thicker, as it is then neceſſary to make a greater exertion to raiſe and depreſs the churn ſtaff. This obſervation is ſo true, that a perſon who has churned butter for half an hour at the beginning, without experiencing any fatigue, cannot perform the ſame work for the ſpace of ten minutes after the cream has become thick and offers more reſiſtance.

Convinced that every interruption, or even diminution of ſpeed, are highly injurious to the butyraceous coagulation, I thought that, without altering either the churn or churn ſtaff, I could adopt a method which is already uſed in ſeveral other operations. Some members of the ſociety think they have already ſeen this application to the churn ſtaff, but not finding it mentioned in any book, I have preſumed, that the ſociety will not object to make it ſufficiently public, in order that it may be adopted in all dairies.

Nothing more is required than to fix to one of the beams of the ceiling, a pole three yards long, ſupported at the diſtance of two feet by a croſs piece, and itſelf faſtened at the end by two blocks or cleats nailed to the timbers. This pole may be about three inches or more in diameter at the upper part, and half that thickneſs at the lower or moveable end. At the end of the pole is tied a ſtrong cord, which is paſſed through a hole made near the top of the churn ſtaff, which is ſo faſtened and placed, that it continues ſuſpended in the middle of the churn.

When the churn ſtaff thus fitted up is preſſed downward, it enters eaſily into the cream; but the pole acting as a lever by its elaſticity, raiſes it immediately; this eaſes the arms, for the moment of the greateſt exertion is when you are obliged to raiſe them as high as the forehead to diſengage the churn ſtaff, and immediately force it down again.

Even if this method contributed only to diminiſh the principal part of the aukwardneſs and conſtraint, it would be a great advantage: but I ſhould think this but trifling and incompleat if the contraction of the arm were not remedied. After having conſidered what powers are jointly employed to produce the effect, I concluded that the poſition of the arm ought not to be neglected during this long operation; and ſeeking

ſeeking by trials for the leaſt painful, I determined in favour of that in which the fore arm is employed in the whole motion.

I therefore perforated the handle of the churn ſtaff about twelve inches below the hole through which the cord paſſes that ſuſpends it to the pole, and I there faſtened two ſmall handles of wood of about two centimetres each, which are faſtened together by an iron pin; ſo that the handle of the churn ſtaff reſembles a croſs. The perſon who churns places his hands on the two handles, and by a ſimple preſſure, which is ſo light, that two fingers are ſufficient to force the churn ſtaff to the bottom of the churn, he may churn for two hours without experiencing any real fatigue. For the motion of the elevation which alone demands the greateſt exertion, is performed by the lever of the pole.

I made uſe of alder wood for the pole, as one of the moſt elaſtic and leaſt likely to warp.

We ſee from this deſcription,

1. How little it coſts to eſtabliſh the mechanical help, which only conſiſts in a pole of alder wood of three yards long, and two ſmall wooden handles adapted to the churn ſtaff.

2. How greatly it aſſiſts and increaſes the power of the perſon who churns, by obviating the moſt painful movement, by placing the hands naturally, ſo as to require the motion of the fore arm only, which is evidently the leaſt fatiguing.

I am convinced by the reſult of conſtant practice, that one third of the time is ſaved which is uſually employed in making butter.

IX.

Deſcription of a Machine for rooting up the Stumps of Trees. By CIT. SAINT VICTOR, *Member of the Society of Agriculture, for the Department of the Seine*.*

Inconveniencies of leaving the ſtumps of trees after felling the trunk.

EVERY cultivator is well aware how detrimental the ſtumps of trees are which remain in the ground, which inſtead of being rooted up at the time they were cut down, have been ſawed off cloſe to the ſtump, to ſave the expence of digging them out of the earth.

* Bibliotheque Phyſ. Oeconomique, de Sonini, No. 1.

Inftance.

In an eftate in Savoy, I was fubjected to thefe inconveniences, from the avarice of the proprietor who had felled a great number of large trees, and fawed them off clofe to the roots, thefe were oaks, walnuts, and chefnuts, which grew in the meadows and ploughed grounds, and he difpofed of them as timber in planks and fire wood, without taking any care to have them rooted up at the fame time, which would have been then more eafy, by the common practice of ufing the trunk as a lever.

Attempt to blow them up by gunpowder.

When I came into poffeffion of this eftate, I forefaw that I could by no means avoid rooting up a great number of ftumps that were very injurious to every kind of cultivation; and to avoid the long, troublefome, and expenfive labour of digging great trenches, which are ufually required to be made, in order to bring up the fort of roots, I thought I might employ the force of gunpowder. This attempt perfectly fucceeded, by means of a little machine of iron I had forged, of which I here join a plan, fection, and perfpective view, which fhews the manner of placing it beneath the ftump *.

Defcription of the Machine.

Machine for blowing up the roots of trees.

It confifts of a bar of forged iron, about two feet eight inches long, one inch thick towards the handle, and of two inches towards the breech or platform.

It is a fmall mortar with a plug and handle.

The platform which is circular, is 14 inches in diameter.

This platform ferves as the bafe of the chamber, or furnace of the mine, which is three inches in diameter, and three inches eight lines in the length of its bore.

* The ufe of this ingenious machine ought not to be confined merely to the ftumps of trees in our fields. Licentioufnefs and avarice within the laft five years, have deprived our country of a quantity of woods and thickets, and degraded by immenfe fellings, our nobleft forefts, the incalculable fource of riches for the prefent, and hope for future time. Whether thefe fpaces thus impoverifhed be converted into arable ground or pafture, or whether the imperious neceffity or claims of pofterity fhould require that they fhould again be planted with trees, the invention of Cit. Saint Victor, will be a moft ufeful application to eradicate from the ground thofe roots which would deprive us of a part of its product.

Note of the editor.

The

The ſtopper or tampion which ſerves as a plug to the mine, is of the ſame diameter, to enter within after a ſlight paper or wadding. It is attached by a chain to the gun or mortar, which laſt is eight inches in diameter.

About two inches above is added a ſmall touch hole and pan. The hole is directed in an angle of 45 degrees, and is primed with powder to communicate with the charge with which the chamber is filled up to the ſtopper.

This engine may be caſt even with more facility in braſs or bronze, and in this caſe, it muſt be a little thicker in all its dimenſions, in order to afford a reſiſtance equal to that of the forged iron.

Uſe of the Machine.

Application beneath the ſtump intended to be blown up.

When the machine is charged with powder, a ſmall excavation is made with a pick axe, in the center of the ſtump. The machine is then placed in it, ſo that the plug immediately touches the wood. Care muſt be taken to fill all the vacancies, either with ſtones, or pieces of iron, or wood, more eſpecially beneath the platform of the machine, in order that the exploſion of the powder may have its full effect on the ſtump, of which if neceſſary, the principal roots ſhould firſt be cut, if any appear on the ſurface of the ground near the ſtump, that is to be eradicated.

When the machine is firmly fixed in its place, the priming is put into the pan, a ſlow match applied, the length of which is ſufficient to allow time to retire to a proper diſtance from the exploſion.

By inſpection of the plan and ſection, every one will perceive the utility of this ſimple machine, and it may eaſily be made by any intelligent ſmith.

Delineation and plate.

A. Plate XIII. Fig. 1. Plan of the machine of about two feet three inches long.

B. Plan of the machine, 10 inches high, comprehending the plug.

a. The plug with its cap faſtened to the chain.

b. The chamber for the powder.

C. The touch hole.

The middle figure repreſents the machine placed under the ſtump of a tree *.

* Mr. Knight, ironmonger, of Foſter Lane, has contrived a ſimple apparatus for ſplitting blocks of wood with gunpowder, of which the deſcription will be given in our next.—N.

Method

X.

Method of Secret Writing, by means of a Steganographic Scale.
By J. B. Berard *.

Methods of ſecret writing.

An infinite variety of methods of ſteganography or indecipherable writing has been deviſed; of theſe, ſome are defective in theory, and others inconvenient in practice. It is not my intention to diſcuſs the advantages or inconveniences of all thoſe which are known, it would lead me too far. I ſhall therefore confine myſelf to noticing a few of the principal ones.

Alphabets of convention.

Alphabets of convention afford but little ſecurity; the characters which perform the functions of vowels and conſonants, recur in ſuch ſituations, as to make their uſe very apparent; and it is aſcertained, that by patience and a little ſkill the ſecret is eaſily diſcovered.

Sympathetic ink.

The ſympathetic inks, ſeven in number, are not more ſafe, becauſe after ſeveral trials, that re-agent which renders the writing legible, is at length diſcovered.

Folding the paper.

Thoſe methods which depend on folding the paper in a particular manner, as was practiſed by the Spartans, are inconvenient, and afford but little defence againſt curioſity.

Kircher's method.

The method of Kircher, though ſufficiently certain in its principle, is of little value in practice; beſides which, its performance is both tedious and inconvenient: a point wrong placed or omitted, is ſufficient to render the ſecret unintelligible to the correſpondent.

General enumeration.

In the ſixth number of the *Journal de l'Ecole Polytechnique*, p. 382, is a table, in which Cit. Haſſenfratz has claſſed all the modes of correſpondence, whether by writing or otherwiſe. That which I have deviſed, is as follows; it will perhaps be found to poſſeſs the double merit of ſimplicity aud ſecurity †.

Take

* Melanges Phyſico-Mathematique.

† This method has been made uſe of for the private correſpondence of Cit. Forfait, miniſter of the marine, and the colonies, and preſident of the lyceum of arts. There is no inconvenience in mentioning the fact; for the advantage of a good method conſiſts in

Take a rule of pasteboard, wood, ivory, or copper, about one inch broad and seven inches long, divide its edge into 30 equal parts, and between the divisions write the first thirty natural numbers, from 1 to 30 inclusive, in any arbitrary order. The following is the manner of using this rule.

New method by a scale of transposition.

Operation for writing. First make a minute or outline of the secret you wish to send, then place the rule or steganographic scale on the paper intended for the secret, and mark the two ends of the rule by two small lines of ink, the reason of which will be seen below.

The process. To write.

This being done, transcribe the first thirty letters of the minute, writing the first letter opposite to the figure 1, on the rule, the second opposite to the figure 2, the third opposite to the figure 3, and so on to the end.

Bring down the rule and transcribe in a similar manner, opposite to the figures, the next thirty letters of the secret, that is to say, from the thirty-first to the sixtieth inclusive.

Continue thus transcribing in new lines, each consisting of thirty letters, until the whole secret is written.

The punctuation must be carefully inserted to the right of that letter which is on its left in the minute, and a mark like this + must be placed above the last letter of each word, to distinguish it from that which follows, and thus render the reading easy to the correspondent.

Operation of the correspondent. The correspondent who receives the secret with the letters thus misplaced, will be able to transcribe it, and replace the letters in their proper order, by inversing the preceding operation.

To decypher.

For this purpose, he must be provided with a rule similar to that made use of for deranging the letters of the secret. He will place it on the first line, so that the letters correspond with the figures on the rule. This will be easily accomplished by means of the two lines of ink which mark the ends of the rule.

It will then be easy for him to transcribe the letters of the secret in their proper order, beginning with that opposite to

in its capability of being employed by all the world, without diminishing its secrecy for each individual; it will be seen that, in this plan, it is the secret of the series agreed on, and not that of the method, which renders the correspondence safe.

figure

figure 1, next that oppoſite to figure 2, and ſo on to thirty he will then bring down the rule, and proceed in the ſame manner with the ſucceeding lines.

Example.

Example. Suppoſe this to be the ſecret to be ſent.

L'eſcadre mettra à la voile au premier vent favorable; elle ſe rendra à Toulon, où je lui enverrai des ordres ultérieurs.

Suppoſe the ſeries of the ſcale employed ſhould be this,

2. 4. 6. 8. 10. 12. 14. 16. 18. 20. 22. 24. 26. 28. 30. 1. 3. 5. 7. 9. 11. 13. 15. 17. 19. 21. 23. 25. 27. 29.

The anagrammatic writing or tranſpoſition of the ſecret, will be;

```
    +     +       + +                     + +
1ecde e ta lv i e u r meL's a r mtrà a ol ape il
          +   + +      +         +                +
1vnfv r be; le e e d a Ture t a oale l sr nrà ol
      +      +             + + +             +    +
1oojl i ne ri e o d e u t l n, ù e u evr a ds rr s ll
1rer                       é i  u s.                1
```

Obſervations.

The combinations to be decyphered in one ſingle line, are many millions of millions,

1. If we wiſh to form a notion of the ſecurity of this method, let us conſider what a geometer muſt do who undertakes to explain the ſecret.

Every line contains a number of combinations, expreſſed by the product of the numbers 1. 2. 3. .... to 30. that is to ſay, a number expreſſed by 265 followed by 30 cyphers. This number of anagrams is equal to that of the grains of ſand which would encircle the whole world, ſuppoſing each grain to be leſs than one hundredth of an inch in diameter. This, without doubt, is more than ſufficient to diſcourage the moſt perſevering Newton, and it muſt be acknowledged that the difficulty may be conſidered as an abſolute impoſſibility.

and they are ſtill more taken all together.

But this is not all, after the labour of a multitude of ages, this geometer would be no farther advanced; for among the infinity of combinations produced from the thirty letters of one line of the ſecret, he would find an innumerable multitude that might yield a rational meaning, and by what means would he be able to diſcover which of them the author of the ſecret had in view? In ſhort, what muſt be the difficulty where it becomes neceſſary thus to combine all the lines of the ſecret at the ſame time? Here the imagination is loſt.

This method is adapted to numerous correſpondents.

2. In the event of many correſpondents, it would be eaſy for the greater ſecurity to agree upon a different ſeries with each, if the ſcale is made of paſteboard, it may be marked with

with four ſeries, and may ſerve for four correſpondents: if it be of ivory, the ſeries may be changed at pleaſure, by writing the figures with a pencil: the ſcale may even be compoſed of thirty ſmall detached moveable pieces of braſs ſlipped upon a flat iron rod, and ſecured with a nut at one of its ends.

3. It may be eaſily conceived, that the principles of permutation may be varied to infinity, for inſtance, by changing only the order of the ſyllables, the words, or the lines. The above method on which I have fixed, appears to be at once eaſy, certain, and conciſe, and to require the ſimpleſt apparatus. Other permutations.

4. If it be deſired to conceal even the appearance of a ſecret, the method above deſcribed may be employed to write with one of the ſympathetic inks, at the end of an oſtenſible ſubject.

5. *Another method.* Having divided the paper into 100 or 200 ſquares, write in each a letter, a ſyllable, or a word of the ſecret, and alſo one of the numbers of the concerted ſeries: ſeparate and mix the ſquares; the correſpondent will only be required to replace them in their order by means of the figures of the ſeries. Another method by the ſcale,

For the greater ſecurity, the edges of the ſquares ſhould be pared to deſtroy the connection of the adjoining ſides.

6. *Method of intercalations.* Draw on paſteboard, ſome parallel diviſions about half an inch aſunder, pierce them with holes, either equal or unequal, of an arbitrary ſize, and at any diſtance from each other: lay the paſteboard on the paper, and write the ſecret by inſerting ſucceſſively from left to right, a letter, ſyllable, or word in each vacancy: remove the paſteboard and fill up the intervals with inſignificant letters; or it will be better, if time permit, that the letters ſhould form with thoſe of the ſecret, a reaſonable meaning, not liable to be ſuſpected. another by intercalation.

The correſpondent will eaſily read the ſecret through holes in a ſimilar paſteboard.

The inſertion of unmeaning letters may be avoided by uſing three or four paſteboards of equal ſize, but ſo pierced, that when they are laid on each other, all the holes will be cloſed. By writing through each of theſe in ſucceſſion, all the lines will be filled. The correſpondent muſt be provided with ſimilar paſteboards, and will read the ſecret through them by applying them ſucceſſively to the writing.

This

This method is ſhorter than that of the anagrammatic ſcale, and is preferable when there is much to write; but it is leſs ſecure, and requires a more complicated apparatus.

XI.

Deſcription of a Magazine Piſtol, which when loaded is capable of being diſcharged Nine ſucceſſive Times through the ſame Barrel. W. N.

Account of a magazine piſtol.

I AM indebted to the liberality of the Right Honourable Lord Camelford for the accurate drawings of the curious and valuable piece which forms the ſubject of the preſent Memoir, as well as for permiſſion to uſe and examine it in any manner I might think proper. It is of German make; the workmanſhip very good, and it has been uſed by his Lordſhip without any particular care in various parts of the world. Its conſtruction does, in effect, ſhew that its uſe is attended with neither danger nor uncertainty; but I ſhall poſtpone my remarks till I have given the deſcription.

Deſcription by reference to the drawings.

Figs. 1 and 4 in the Plates XIV. and XV. exhibit views of the two ſides of the piſtol. Fig. 5. is a ſection to ſhew the magazines, and Figs. 2. and 3. ſhew what may be called the chamber piece. The large face of this laſt (Fig. 2.) is ſlightly taper or conical from X towards Y, where the diameter is ſmalleſt, and the ſmall part B is cylindrical, excepting that a portion is ſcooped out on one ſide marked by the dark ſpace D in Fig. 3. Its proper place in the piſtol is in the breech, croſſing the line of direction at right angles. When the lock is off, it goes into its cell from the ſide Fig. 1. which it fits with conſiderable accuracy, but does not come to rub hard, becauſe the Plate X forms a projecting face which ſtops it. In this ſituation its ſmall cylindrical part B projects outwards, and is received in an hole of the ſame diameter in that part of the lock beneath the hammer where the pan is uſually placed. In fact, the cavity D, Fig. 3. does conſtitute the pan when its poſition is ſuch as to be immediately beneath the covering face of the hammer. The ſcrew B which goes into the cylindrical piece, and Z, which goes into the ſtock, are the faſtenings by which the lock is ſecured. At the oppoſite end A,

Defcription of a piſtol which fires nine balls in fucceffion by once charging.

A, Fig. 2. the chamber piece terminates in a ſquare, upon which the broad head of the lever L, Fig. 4. is fitted, and kept down by the ſcrew A, which goes into the chamber piece. It is to be underſtood that the cock and hammer are conſtructed, and act in the ſame manner as in the beſt fire arms, and do not therefore require to be deſcribed. The lever is capable of being moved from the poſition M to thoſe of L and N; but is prevented from deſcribing the remaining part of the circle by an interior ſtop, which may be eaſily imagined without any attempt at minute explanation.

Fig. 5. being a ſection through the middle of the ſtock, breech, and part of the barrel, ſhews the relative ſituation of the magazines, with the chamber piece, and other parts. The balls S, 2, 3, 4, &c. are lodged in their proper receptacle, being put in through the hole R, Fig. 4. and the powder is lodged in its magazine Q Q. both which are cloſed by the door T, which is then faſtened by a ſmall bolt and back ſpring. A, Fig. 5. ſhews the ſituation of the chamber piece at the time when the lever is brought to the poſition M. This is done with the muzzle of the piece pointing towards the ground, and the effect is, that powder runs into the chamber P (ſee alſo Figs. 2 and 3) and a ball into the chamber S. The lever is then moved from M to L and N, by which proceſs the ball chamber drops its ball into the barrel as it paſſes oppoſite to N, where the ball remains, becauſe the actual bore of the ſcrew barrel is not wide enough to allow it to paſs farther than juſt to clear the moveable chamber. As ſoon as the lever has arrived at the poſition N, the powder chamber P is exactly oppoſite the ball, and ready to be diſcharged againſt it. After the diſcharge the muzzle is to be again depreſſed, and the lever moved from N to M: the chambers become again charged with powder and ball; and by returning the lever back to N, this ball and powder become duly placed for a ſecond diſcharge. It is obvious that theſe diſcharges may be repeated until all the balls have been fired out. The ſmall bridge in the powder chamber P (Fig. 2.) is to prevent any impediment from the entrance of part of one of the balls into the receptacle, and the perforations W, W, in the breech, ſerve to clear the ſurface of the chamber piece from any foulneſs it might acquire.

Thus

Defcription of a piftol which fires nine balls in fucceffion by once charging.

Thus far we have fpoken only of loading and difcharging; but this piece would admit of little rapidity of effect, if it did not at the fame time cock and prime itfelf. It may be obferved, that the projecting part, or ftud C, is fixed to the chamber piece Fig. 2. and 3. a very little behind the fhot chamber; fo that it ftands at the top of the lock at the time of charging. A thin flat bar of iron proceeds from the cock, and flides with it along the face of the lock plate, withinfide the hammer: againft this plate the ftud C acts, and brings it to full cock a little before the lever arrives at the pofition M; at the fame time that the more prominent part of C preffes on the back of the hammer, and fhuts the pan. The lock is therefore put into the condition to give fire by the fame fimple operation, and precifely at the fame moment as the charge is taken from the magazines.

It now remains only to be fhewn how the priming is given. The excavation D in the fide of the fmall cylinder B, Fig. 3. conftitutes the pan, and the fmall dot reprefents the touch hole paffing through the metal into P the powder chamber. When the lever is in the pofition M, Fig. 4, the cavity D is exactly placed beneath the covering face of the hammer G: but at the time of charging it has the pofition Fig. 3. confidered with regard to that of Fig. 1. In this laft figure the dark fhaded fpace H denotes a refervoir for priming, the door of which may be opened and fhut under the action of an ingenious back fpring, operating nearly like that of a clafp knife. A long perforation or flit communicates from this refervoir to the fpace in which the cylinder B revolves: fo that when the excavation D paffes that flit (during the return of the lever) it becomes filled with powder, which it carries round to the laft pofition, which is exactly that in which it muft receive the fire whenever the piftol is to be difcharged.

The hiftory of this conftruction feems to be imperfect; but there is reafon to fuppofe that it is of fome antiquity. The magazine air gun of *Colbe* conftructed for carrying ten balls, and lodging them fucceffively in the barrel by a crofs cylindrical piece was made early in the laft century, and is defcribed in Defagulier's Lectures, and moft elementary books. In the prefent arm the contrivances are highly judicious, as well with regard to mechanifm as to arrangement. If it were poffible for the powder magazine to be fet on fire at the diftance

tance of a femi-circumference of the chamber piece from the explofion, the only effect would be that the door would be blown open, and this is fituated in a place where it could do no harm. The fame remark is alfo applicable to the magazine for priming.

Defcription of a piftol which fires nine balls in fucceffion by once charging.

How great the advantages muft be in battle, for a man to be able to reload his piece by a fimple movement of one fecond of time, without taking his eye off his enemy; or how confiderably ufeful this invention might prove in the defence againft robbers need not be ftated. It can indeed be ftated, that the opponent may alfo provide himfelf with the like advantage; and then we have only to urge the argument, that the duration of wars have been diminifhed, and its humanity diminifhed by rendering the means of annoyance more perfect.

I have difcharged this courfe of balls feveral times, and I find that the whole nine balls can be fired in 30 feconds.

XII.

On the Diffemination of Plants. *By* CIT. L. REYNIER *.

THE hiftory of vegetables affords fome facts to which I think it my duty to call the attention of naturalifts: they relate to the diffemination of plants, and to the means by which this is effected. I have already collected feveral obfervations on this fubject in the dictionary of Agriculture of *L'Encyclopédie Méthodique,* article *Diffemination.*

Diffemination of plants,

There are two natural means of reproduction: one of thefe is by the roots, which, fpreading outwards, form new ftems round the mother plant; this reproduction is flow, and can only take place gradually and without intervals; the other is by the feeds, which being carried by the winds, or by the hooks with which they are provided, or by animals which fwallow them, and afterwards depofit them, unchanged, in their excrements, are removed to greater diftances, though ftill within a limited circle. It is not therefore wonderful, to fee a plant fpring up in a fpot, where the fame fpecies is known to exift at no great diftance; its introduction is in the clafs of poffibilities.

by the fpreading of the roots, or tranfportation of feeds.

* Decade Philofophique, No. 13. An. XI.

But

New vegetations that appear when the face of the ground is difturbed.

But that which ought to excite our attention, is the production of a new vegetation in a place which has undergone fome very great changes. In all cafes when the furface of a place has been altered either by the falling or removal of the earth, by opening roads through forefts, or by draining marfhes, plants are fure to be found in the following year, which did not exift there before, while the former fpecies difappear, except a few cofmopolites, if the expreffion may be allowed, which, though they re-appear, undergo a remarkable alteration *(Dict. d'Agr. de L'Encycl. meth.* art. *Climat.)* When a wood is grubbed up, the foreft plants ceafe to grow, and thofe natural to tilled ground immediately germinate. Labat notices (fee Vol. I. p. 386.) that, in his time, at the Antilles, as foon as the ground was cleared, its furface became covered with purflane. In thofe places where charcoal is made, in forefts at a great diftance from any habitation, I daily obferve plants, different from thofe which were formerly there, and which are natives either of dry paftures or cultivated lands, fuch as the (vergerette) of Canada, the annual veronicas, &c. Forfter, in his voyages with Cook, took notice of feveral iflets in the middle of the fea, fimply rocks of coral, on which the firft rudiments of vegetation were forming. In the low valleys of the downs of Holland, I have gathered a fatyrion and an ophrys, natives of the fummits of the Alps and the plains of Spitzbergen *(Satyrium viride, L. Ophrys monorchis, L.)* But thefe downs, according to the obfervations of Cit. Décandolle, *(Ann, d'Agr. T. XIII. p. 372.)* are not of very ancient origin. How is the introduction of thefe two plants, which are naturally provided with very limited means of multiplication, to be accounted for? I have expreffed a wifh in the Encyclopedie, that the new iflands formed by volcanos, and particularly thofe near Santorini fhould be examined. One of our moft accurate obfervers is on the point of vifiting the latter, *(See d'Olivier, T. II. p. 238).* The new one produced by the eruption of 1707 to 1711, does not yet fhew any appearance of vegetation, the air is ftill mephitic; that which arofe in 1573 has fome traces of vegetation, particularly fome graffes, and a fmall fig tree; the latter may have been depofited by fome bird, but by what means were the graffes conveyed?

Are thefe produced by the roots or feeds of other plants.

4 It

It is true that ſeeds which are buried to a certain depth will be preſerved for a great length of time, and being afterwards ſubmitted by culture to the action of the light and water, will germinate. But is it to be preſumed, that the ſeeds of purſlane were preſerved in the earth of the Antilles, during the many ages the foreſts had endured before they were deſtroyed? Is it probable that, in the foreſts of Gaul, whoſe antiquity is well aſcertained, the ſeeds of thoſe plants, which appeared after they were rooted up, or after placing a charcoal furnace in them, ſhould have been preſerved ſound from before the exiſtence of the foreſts? The diſſemination of plants has neceſſary limits; beyond theſe, we muſt have recourſe to other explanations. Could the ſeeds have been ſo long preſerved.

Laſtly, the plants of the claſs cryptogamia, and particularly fungi, whoſe form is conſtantly determined by the nature of the ſubſtance on which they grow, and is conſtantly the ſame in ſimilar circumſtances, although they do not appear at ſtated ſeaſons like other vegetables, alſo preſent new facts. It is not long ſince Cit. Ventenat obſerved a boleta of a peculiar new ſhape, which was produced on a human body. I have noticed the clavaria, which is conſtantly formed on the cryſalides of caterpillars (*Journal de Phyſ. Année.* 1787); others grow on thoſe fruits which have a woody covering; ſuch as are formed on the fragments of fir, are not the ſame as thoſe which grow on the wood of the oak. The huſk of the nutmeg produces a fungus peculiar to itſelf, (*Ancienne Encycl.* art. *Muſcade*). Laſtly, the fungi which are formed on the wooden props of mines, cellars, &c. are not the ſame as thoſe produced by the ſame woods when they decay in the open air. It is difficult to conceive the diſſemination of ſeeds from one mine to another, particularly to thoſe which are opened in ſituations where no mines had exiſted before; nevertheleſs I have found all thoſe which I have examined in different countries to poſſeſs very nearly the ſame form. Facts reſpecting the cryptogamia.

All theſe conſiderations may induce us to preſume, that nature is daily exerciſing the ſame powers which ſhe poſſeſſed at the creation, and it would be intereſting to examine the cauſes and aſcertain the means of execution; but this can only be the conſequence of long and diverſified inveſtigations, carried on by a great number of obſervers. It is preſumed that plants continue to be formed without propagation.

XIII.

An Essay on the Declivities of Mountains. By RICHARD KIRWAN, *Esq. L. L. D. F. R. S. and P. R. I. A,*

Introductory remarks.

AMONG the various causes to whose activity the planet we inhabit owes its present wonderfully diversified appearance, some undoubtedly exerted their influence from its very origin, and others at subsequent periods; of these last one at least, namely, the Noachian deluge, was universal in its operation, while the effects of many more were partial and local, such as those resulting from earthquakes, volcanos, particular inundations, &c.

In a general survey of the globe, it is only to general causes, whose operation was universal, that our attention can be directed; the effects of partial causes being the proper objects of the geological history of those countries that were particularly affected by them.

But to distinguish causes of the former class from those whose operation was more confined, it is necessary to discover some character by which their effects may unequivocally be discerned.

Now a general *uniformity,* or agreement in some particular circumstance in every part of the globe, seems to be a sure test of the operation of some general cause. The discovery of uniform appearances is therefore of primary importance in geological researches. In the present essay I shall confine myself to the investigation of one instance of this sort, namely, the *inequality of declivity* which the sides or flanks of mountains exhibit in every part of the globe hitherto examined according to the points of the compass to which they face, and are exposed.

All high mountains or hills have one side much steeper than the other.

That one part of almost every high mountain or hill is steeper than another, could not have escaped the notice of any person who had traversed such mountains; but that Nature in the formation of such declivities had any regard to different aspects or points of the compass, seems to have been first remarked by the celebrated Swedish geologist Mr. Tilas, in the 22d vol. of the Memoirs

Memoirs of Stockholm for 1760*. Neither Varenius, Lulolph nor Buffon in his Natural Hiſtory publiſhed in 1748, have noticed this remarkable circumſtance.

The ſteep ſide faces the low country. Tilas.

The obſervation of Tilas however relates only to the extreme ends, and not to the flanks of mountains; with reſpect to the former, he remarked that the *ſteepeſt* declivity always faces that part of the country where the land lies loweſt, and the *gentleſt* that part of the country where the land lies higheſt, and that in the ſouthern and eaſtern parts of Sweden they conſequently face the E. and S. E. and in the northern the W. The eſſential part of this obſervation extends therefore only to the general elevation or depreſſion of the country, and not to the bearings of theſe declivities.

The weſtern ſide is ſteepeſt. Bergmann.

The diſcovery that the different declivities of the flanks of mountains bear an invariable relation to their different aſpects, ſeems to have been firſt publiſhed by Mr. Bergman in his phyſical deſcription of the earth, of which the ſecond edition appeared in 1773. He there remarked that in mountains that extend from N. to S. the weſtern flank is the *ſteepeſt*, and the eaſtern the *gentleſt*. And that in mountains which run E. and W. the ſouthern declivity is the ſteepeſt and the northern the gentleſt, vol. 2d. § 187.

Inſtances. In Scandinavia.

This aſſertion he grounds on the obſervations related in his firſt vol. § 32, namely, that 1° in Scandinavia the Suevoberg mountains that run N. and S. ſeparating Sweden from Norway, the weſtern or Norwegian ſides are the ſteepeſt, and the eaſtern or Swediſh the moſt moderate, the verticality or ſteepneſs of the former being to that of the latter as 40 or 50 to 4 or 2 †.

The Alps.

2dly. That the Alps are ſteeper on their weſtern and ſouthern ſides than on the eaſtern and northern.

The Cordelieres.

3dly. That in America the Cordelieres are ſteeper on the weſtern ſide, which faces the Pacific Ocean, than on the eaſtern. But he does not notice a few exceptions to this rule in particular caſes which will hereafter be mentioned.

Remarks of Buffon,

Buffon, in the firſt vol. of his Epochs of Nature publiſhed in 1778, p. 185, is the next who notices the general prevalence

* See alſo vol. 25, Swed. Abhandl. p. 291, where *Cronſted* explains ſome obſcure parts of Tilas's obſervation.

† The verticality of the ſides is inverſely as the length of the deſcent.

of this phænomenon, as far as relates to the eaſtern and weſtern ſides of the mountains that extend from north to ſouth, but he is ſilent with reſpect to the north and ſouth ſides of the mountains that run from eaſt to weſt; nay, he does not ſeem to have had a juſt comprehenſion of this phenomenon, for he conſiders it conjointly with the general dip of the regions in which theſe mountains exiſt. Thus he tells us, vol. 1ſt, p. 185, that in all continents the general declivity, taking it from the ſummit of mountains, is always more rapid on the weſtern than on the eaſtern ſide; thus the ſummit of the chain of the Cordelieres is much nearer to the weſtern ſhores than to the eaſtern; the chain which divides the whole length of Africa, from the Cape of Good Hope to the mountains of the Moon, is nearer, he ſays, to the weſtern than to the eaſtern ſeas; of this however he muſt have been ignorant, as that tract of country is ſtill unknown.

The mountains which run from Cape Comorin through the peninſula of India are, he ſays, much nearer to the ſea on the eaſt than on the weſt; he probably meant the contrary, as the fact is evidently ſo, and ſo he ſtates it in the 2d vol. p. 295; the ſame he tells us may be obſerved in iſlands and peninſulas, and in mountains.

and other authors;

This remarkable circumſtance of mountains was notwithſtanding ſo little noticed, that in 1792 the author of an excellent account of the territory of Carlſbad in Bohemia, tells us he had made an obſervation, which he had never met with in any phyſical deſcription of the earth, namely, that the ſouthern declivity of all mountains was much ſteeper than the northern, which he proves by inſtancing the *Erzgebirge* of Saxony, the Pyrenees, the mountains of Switzerland, Savoy, Carinthia, Tyrole, Moravia, the Carpathian and Mount Hæmus in Turkey, 2, Bergm. Jour. 1792, p. 385, in the note.

Herman;

Herman in his Geology, publiſhed in 1787, p. 90, has at leaſt partially mentioned this circumſtance, for he ſays that the eaſtern declivities of all mountains are much gentler and more thickly covered with ſecondary ſtrata, and to a greater height, than the weſtern flanks, which he inſtances in the Swediſh and Norwegian mountains, the Alps, the Caucaſian, the Appenine and Ouralian mountains; but the declivities bearing a ſouthern or northern aſpect he does not mention.

La Metherie;

La Metherie, in the 4th vol. of his Theory of the Earth, of which the ſecond edition appeared in 1797, a work which

 abounds

abounds in excellent obſervations, p. 381 *, produces numerous inſtances of the inequality of the eaſtern and weſtern declivities, but ſcarce any of the northern and ſouthern, whoſe difference he does not ſeem to have noticed, but he makes a remark which I have not ſeen elſewhere, that the coaſts of different countries preſent ſimilar declivities.

With regard to eaſtern and weſtern aſpects, he thinks that a different law has obtained in Africa from that which has been obſerved in other countries, for in that vaſt peninſula he imagines the eaſtern declivities of mountains are the ſteepeſt, and the weſtern the gentleſt. Of this however he adduces no other proof, but that the greateſt rivers are found on the weſtern ſide: this proof ſeems inſufficient, as, if mountains be ſituated far inland, great rivers may flow indiſcriminately from any ſide of them, and ſometimes few rivers flow even from the ſide whoſe deſcent is moſt moderate, for inſtance, from the eaſtern ſide of the mountains of Syria; the Elbe and the Oder, two of the greateſt rivers in Germany, take their courſe from the weſtern ſides, the firſt of the Bohemian and the other of the Moravian mountains, which yet are the ſteepeſt. Many originate from lakes, as the Shannon with us; many take ſuch a winding courſe, that from a bare knowledge of the place of their diſemboguement it is impoſſible to judge from what ſide of a mountain they iſſue, if from any; their courſe at moſt diſcovers the depreſſion of the general level of the country.

J. R. Foſter. Univerſal fact: that the S. and S. E. ſides of mountains are ſteepeſt.

In 1798, the celebrated traveller and circumnavigator, John Reinhold Foſter, publiſhed a geological tract which merits ſo much more attention, as all the facts were either obſerved by himſelf, or related to him by the immediate obſervers. In this he ſtates as a fact univerſally obſerved, that the ſouth and ſouth-eaſt ſides of almoſt every mountain are ſteep, but that the north and north-weſt ſides are gently covered and connected with ſecondary ſtrata in which organic remains abound, which he illuſtrates by various inſtances, ſome of which have been already, and others will preſently be mentioned.

At preſent this fact attracts the greateſt attention, being obviouſly connected with the original ſtructure of the globe, and clearly proving that mountains are not mere fortuitous eruptions unconnected with tranſactions on the ſurface of the earth, as has of late been confidently advanced.

* It is to be regretted that he ſcarce ever quotes his authorities.

 I ſhall

I ſhall now ſtate the principal obſervations relative to this object, that have been made in different parts of the world.

In Europe.

Account of mountains: in Europe,

1. The mountains that ſeparate Sweden from Norway extend from north to ſouth, their weſtern ſides are *ſteep* and the eaſtern *gentle*, 1. Bergm. Erde Beſchreib. p. 157.

2. The Carpathian mountains run from E. to W. their ſouthern ſides towards Hungary are ſteep, their northern towards Poland moderate, Foſter, § 46.

3. Dr. Walker, profeſſor of natural hiſtory at Edinburgh, obſerved that the coaſts and hills of Scotland are ſteeper and higher on the weſtern ſide than on the eaſtern; Jamiſon's Mineralogy of Shetland, p. 3. However, Jamiſon obſerved, that the ſouth ſide of the iſle of Arran is the loweſt, and the north ſide the higheſt, p. 51.

4. The mountains of Wales are gentle on the eaſtern and ſteep on the weſtern ſides.

5. The mountains of Parthery, in the county of Mayo, are ſteep on the weſtern ſide.

6. The mountains which ſeparate Saxony from Bohemia deſcend gently on the Saxon or northern ſide, but are ſteep on the Bohemian or ſouthern ſide; Charpent, p. 75. The ſouthern declivity is to the northern as ſix to two, 2d Bergm. Journ. 1792, p. 384 and 385.

7. The mountains which ſeparate Sileſia from Bohemia run nearly from E. to W. yet are ſteeper on the northern or Sileſian ſide than on the oppoſite Bohemian; Aſſemanni *Sileſia*, 335. Such branches as run from N. E. to S. W. have their weſtern covered with primordial ſtrata, and conſequently leſs ſteep; 4. New Roz. p. 157.

8. The Meiſſener in Heſſia is ſteeper on the N. and E. ſides which face the Warra, than on the ſouth and weſtern; 1. Bergm. Journ. 1789, p. 272.

9. The mountains of the Hartz and Habichtſwald are ſteep on the ſouth and gentle on the northern ſides, Foſter, § 46.

10. The Pyrenees, which run from E. to W. are ſteeper on the ſouthern or Spaniſh ſide; Carbonieres, XIII.

11. The mountains of Crim Tartary are gentle on the northern and ſteep on the ſouthern ſides, Foſter, ibid.

In

In Aſia.

12. The Ourals, which ſtretch from N. to S. are far ſteeper on the weſtern than on the ſouthern ſides; Herman Geologie, p. 90, and 2d Ural Beſchreib. p. 389. In Aſia,

13. The mountain of Armenic to the weſt of the Ourals is ſteep on its E. and N. ſides, but gentle on the ſouthern and weſtern; 1. Pallas Voy. p. 277.

14. The Altaiſchan mountains are ſteep on their ſouthern and weſtern ſides, but gentle on the northern and eaſtern; Foſter, ibid. and Herman 2d. Ural Beſchreib. p. 390, in the note.

15. So alſo are the mountains of Caucaſus, 3d. Schrift. Berl. Geſelſch. 471.

16. The mountains of Kamſkatſka are ſteep on the eaſtern ſides, Pallas, 1 Act. Petropol. 1777, p. 43.

17. The Ghauts in the Indian peninſula are ſteep on the weſtern ſides.

18. The mountains of Syria which run from N. to S. ſkirting the Mediterranean, are ſaid to be ſteeper on the weſtern ſide facing the Mediterranean; 4. La Metherie, p. 380.

In America.

The Cordelieres run from N. to S. their weſtern flanks towards the Pacific are ſteep, their eaſtern deſcend gradually. In America:

In Guiana there is a chain of mountains that run from E. to W. their ſouthern flanks are ſteep, their northern gentle; Voyages de Condamine, p. 140.

To aſſign the cauſes of this almoſt univerſal allotment of unequal declivities to oppoſite points, and why the greateſt are directed to the weſt and ſouth, and the gentleſt on the contrary to the eaſt and north, it is neceſſary to conſider, Deduction of cauſes.

1. That all mountains were formed while covered with water.

2. That the earth was univerſally covered with water at two different æras, that of the creation, and that of the Noachian deluge. The earth was originally covered with water.

3. That in the firſt æra we muſt diſtinguiſh two different periods, that which preceded the appearance of dry land, and that which ſucceeded the creation of fiſh, but before the ſea had been reduced nearly to its preſent level; during the former Two æras.

former, the primæval mountains were formed, and during the laſt, moſt of the ſecondary mountains and ſtrata were formed.

4. That all mountains extend either from E. to W. or from N. to S. or in ſome intermediate direction between theſe cardinal points, which need not be particularly mentioned here, as the ſame ſpecies of reaſoning muſt be applied to them, as to thoſe to whoſe aſpect they approach moſt.

In the firſt the primæval mountains were formed, and the waters moved from E. to W. and from N. to S.

Theſe preliminary circumſtances being noticed, we are next to obſerve that during the firſt æra, this vaſt maſs of water moved in two general directions, at right angles with each other, the one from E. to W. which needs not to be proved, being the courſe of tides which ſtill continue, but were in that ocean neceſſarily ſtronger and higher than at preſent: the other from N. to S. the water tending to thoſe vaſt abyſſes then formed in the vicinity of the ſouth pole, as ſhewn in my former eſſays. Before either motion could be propagated, a conſiderable time muſt have elapſed.

The primæval mountains reſiſted the waters, which occaſioned the greateſt depoſitions on the N. and E. ſides.

Now the primæval mountains formed at the commencement of the firſt æra, and before this double direction of the waters took place, muſt have oppoſed a conſiderable obſtacle to the motion of that fluid in the ſenſe that croſſed that of the direction of theſe mountains. Thus the mountains that ſtretch from N. to S. muſt have oppoſed the motion of the waters from E. to W. this oppoſition diminiſhing the motion of that fluid diſpoſed it to ſuffer the earthy particles with which in thoſe early periods it muſt have been impregnated, to cryſtallize or be depoſited on theſe eaſtern flanks, and particularly on thoſe of the higheſt mountains, for over the lower it could eaſily paſs; theſe depoſitions being inceſſantly repeated at heights gradually diminiſhing as the level of the waters gradually lowered, muſt have rendered the eaſtern declivities or deſcent, gentle, gradual, and moderate, while the weſtern ſides receiving no ſuch acceſſions from depoſitions, muſt have remained ſteep and craggy.

Again, the primæval mountains that run from E. to W. by oppoſing a ſimilar reſiſtance to the courſe of the waters from N. to S. muſt have occaſioned ſimilar depoſitions on the northern ſides of theſe mountains againſt which theſe waters impinged, and thus ſmoothed them.

Where

Where mountains interſect each other in an oblique direction, the N. E. ſide of one range being contiguous to the S. W. flanks of another range, there the afflux of adventitious particles on the north-eaſt ſide of the one, muſt have frequently extended to the S. W. ſide of the other, particularly if that afflux were ſtrong and copious; thus the *Erzgebirge* of Saxony, which run from W. to E. have their N. E. ſides contiguous to the S. W. ſide of the *Rieſengebirge* that ſeparate Sileſia from Bohemia, and hence theſe latter are covered with the ſame beds of gneiſs, &c. as the northern ſides of the Saxon, and thereby are rendered ſmooth and gentle comparatively to the oppoſite ſide, which being ſheltered, remains ſteep and abrupt, which explains the ſeventh obſervation.

Mountains near each other would intercept this; depoſition.

The cauſes here aſſigned explain why the covering of adventitious ſtrata on the higheſt mountains is generally thinneſt at the greateſt height, and thickeſt towards the foot of the mountain, for the bulk of the water that contained the adventitious particles being proportioned to its depth, and the maſs of earthy particles with which it was charged, being proportioned to the bulk of water that contained them, it is plain, that as the height of water gradually decreaſed, the depoſitions from it on the higher parts of the mountains muſt have been leſs copious than on the lower, where they muſt have been oftener repeated.

It would be leaſt at the greateſt heights, &c.

Hence, 2. granitic mountains, generally the moſt ancient, frequently have their northern or eaſtern ſides covered with ſtrata of gneiſs or micaceous ſhiſtus, and this often with argillite, or primæval ſand-ſtone, or lime-ſtone, theſe being either of ſomewhat later formation or longer ſuſpendible in water.

Hence, 3. different ſpecies of ſtone are often found at different heights of the ſame flank of a mountain, according as the water which conveyed theſe ſpecies, happened to be differently impregnated at different heights; during the firſt æra its depoſitions formed the primitive ſtony maſſes, but after the creation of fiſh, lime-ſtone, ſand-ſtone, farcilites and ſecondary argillites, in which piſcine remains are found, were depoſited. But during the ſecond æra, viz. that of the Noachian deluge, by reaſon of the violence and irregularity of its aggreſſion, the depoſitions were more miſcellaneous and are found at the greateſt heights; yet in general they may well

well be diſtinguiſhed by the remains of land animals, or of vegetables, or of both, which they preſent in their ſtrata (or at leaſt by the impreſſions of vegetables which they bear) as theſe muſt have been conveyed after the earth had been inhabited. But mountains regularly ſtratified bearing ſuch remains, for inſtance the carboniferous, cannot be deemed to have been formed in a period ſo tumultuous. During this deluge the waters alſo held a different courſe, proceeding at firſt from ſouth to north, and afterwards in both oppoſite directions, as ſhewn in treating of that cataſtrophe in my ſecond eſſay.

Exceptions from local cauſes.

Hence, and from various contingent local cauſes, as partial inundations, earthquakes, volcanos, the eroſion of rivers, the elapſion of ſtrata, diſintigration, the diſruption of the lofty mounds by which many lakes were anciently hemmed in, ſeveral changes were produced in particular countries that may at firſt ſight appear, though in reality they are not, exceptions to the operations of the general cauſes already ſtated.

Inſtances.

Thus the mountains of Kamſkatſka had their eaſtern flanks torn and rendered abrupt by the irruption of the general deluge, probably accompanied by earthquakes. And thus the Meiſſener had its E. and N. flanks undermined by the river Warra, as Werner has ſhewn; thus the eighth and ſixteenth obſervations are accounted for, as is the thirteenth, by the vaſt inundations ſo frequent in this country, 1 Pallas, p. 172, which undermined or corroded its E. ſide, while the weſtern were ſmoothed by the calcareous depoſitions from the numerous rivers in its vicinity.

Different ſpecies of ſtone muſt be found on different ſides of mountains.

Hence, 4. we ſee why on different ſides of lofty mountains different ſpecies of ſtones are found, as Pallas and Sauſſure have obſerved, 2 Sauſſ. § 981, a circumſtance which Sauſſure imagined almoſt inexplicable, but which Dolomieu has ſince happily explained, by ſhewing that the current which conveyed the calcareous ſubſtances to the northern, eaſtern, and north-eaſtern ſides of the Alps, for inſtance, was ſtopped by the height of theſe mountains, and thus prevented from conveying them to the ſouthern ſides, and thus the north-eaſtern ſides were rendered more gentle than the oppoſite, 3. New Rotz. p. 425. conformably to the theory here given.

Interceptions.

Hence, 5. where ſeveral lofty ridges run parallel to each other, it muſt frequently happen that the external ſhould intercept

tercept the depoſitions that do not ſurmount them, and thus leave the internal ridges ſteep on both ſides.

Low mountains.

Hence, 6. low granitic or other primitive hills are frequently uncovered by adventitious ſtrata on all ſides, as at Phanet in the county of Donegal, or are covered on all ſides; the impregnated waters either eaſily paſſing over them, or ſtagnating upon them, according to the greater or leſs rapidity of its courſe, and the obſtacles it met with.

Remarks on the ſtatements of Buffon and Bergmann.

The twofold motion of the ancient ocean is noticed both by Buffon and Bergman, but neither of them have deduced from it the true explanation of the phenomena of which we here treat: Buffon attributes the formation of ſecondary mountains to depoſition or ſediments from the ſea after the exiſtence of fiſh, 1. Epoques, p. 143, in 8vo. which he ſays inveſted the baſes of mountains without noticing any diſtinction of ſides, p. 144 and 170. He thinks theſe ſediments were equally conveyed from both poles towards the equator, for it is the equatorial regions that he thinks thoſe mighty caverns opened, towards which the primitive ocean was impetuouſly borne and in which it was ingulphed, p. 181, 182, and 183. If ſo, ſimilar declivities ſhould be formed on the ſouthern as on the northern ſides of mountains, which is contrary to the obſerved facts. His explanation of the eaſtern and weſtern declivities is defective and erroneous, for he attributes the abruptneſs of the weſtern ſides to the eroſion of the coaſts on that ſide (an eroſion that exiſts only in fancy) and the ſmoothneſs of the eaſtern to the gradual deſertion and retreat of the ſea on that ſide, p. 184. and 185, a retreat equally fictitious, as De Luc has well ſhewn. Whereas ſince the general motion of the ſea is from E. to W. if the eroſion were of either ſide it ſhould rather be on the eaſtern than on the weſtern; beſides, if the gentle declivities of the eaſtern ſides of mountains aroſe from the gradual retreat of the ſea, the petrifactions of the ſecondary mountains thus formed ſhould conſiſt of ſuch ſhell-fiſh as inhabit ſhallow ſeas or ſhores, whereas they conſiſt chiefly of thoſe called *pelagicæ*, which inhabit the greateſt depths *.

With reſpect to the eaſtern and weſtern declivities, Mr. Bergman's account of the origin of their inequality agrees exactly with mine, 2. Bergm. Erdeklotet § 183 and 187, but he

* 2. Bergm. Erdekugel, p. 315.

fails

fails in accounting for the inequality of the northern and southern, for he ſuppoſes the courſe of the water to tend equally from both poles towards the Æquator which would render the depoſitions equal on both ſides, which is contrary to obſervation.

XIV.

A Memoir on Animal Cotton, or the Inſect Fly-Carrier. By M. Baudry des Lozieres, *Member of ſeveral Academies, and Founder of the Society of Sciences and Arts, at Cape François.* (American Tranſ. V.)

Gentlemen,

Preface.

Before I enter upon the ſubject of this memoir, I ought to pay the tribute of praiſe which is due to due to your uſeful labours. But the ſtyle of eulogy is ill ſuited to the plainneſs of an American farmer, and while you are conſtantly employed in *deſerving* praiſe, you cannot ſpare time to *hear* it.

Propoſed new branch of Weſt India commerce.

I am now going to communicate to you, with ſome obſervations upon it, a fact of entomology which I have myſelf witneſſed during my reſidence at St. Domingo, and which, if I am not miſtaken, deſerves your greateſt attention, becauſe it may introduce a new branch of commerce with the Weſt India colonies, and render very uſeful an animal which has hitherto been known only by the miſchief which it occaſions.

Every Inhabitant of the Weſt Indies knows and dreads the greedy worm which devours their indigo and caſſada plantations. But people have hitherto turned their attention more to the means of deſtroying it than of rendering it uſeful. It is indeed very natural to endeavour to deſtroy our enemy, but to compel him to be of ſervice to us is by far the greater triumph.

Its Birth, Growth, and Death.

Production, growth, and death of the caſſada worm.

The caſſada worm is produced like the ſilk worm, that is to ſay, from the eggs which the mother ſcatters every where, after ſhe has undergone her metamorphoſis into a whitiſh butterfly, or of a light pearl colour.

The

The egg is hatched about the latter end of July. Its developement is quick, for in September the worm is changed into a butterfly.

This month of September is the ſeaſon of his loves. The conſtant motion of his wings ſhews the ardency of his paſſion which he indulges day and night and even while feeding. The exceſs of this indulgence ſoon deſtroys him, he dies in the ſame month after violent convulſions.

I have ſaid that his life begins at the end of July. He is decked at his birth with a robe of the moſt brilliant variegated colours. This elegant livery, which nature ſeems to have delighted in forming, renders him always agreeable to the eye, which always dwells upon it with pleaſure.

Its Affinities.

It has appeared to me to be a ſmooth caterpillar whoſe external ſhape is exactly like that of the ſilk worm. External Appearances, &c.

It differs however from it, by its ſize, by its thickneſs, and by the beauty of its colours.

It again differs from the ſilk worm, becauſe it does not itſelf work the cone which I am going to ſpeak of.

I leave it to the learned to delineate its external configuration, and to determine upon the family of inſects to which it belongs. I ſhall only ſay that I do not believe it has, like the ſilk-worm, an inteſtine going in a direct line from the mouth to the anus, becauſe it appears to me that this cauſe of elaboration would not have the ſame deſtination.

Its Food.

It feeds on caſſada leaves, of which it is extremely greedy. Food. It feeds at all hours, day and night. It alſo nibbles the leaves of the potatoe, this is however but a tranſitory taſte, it ſoon returns to the caſſada leaf.

I have to obſerve that after it has taken its food, when the time of its metamorphoſis arrives, it does not purge itſelf by diet, like the ſilk worm, but continues to eat to the laſt moment.

The Approach of its Metamorphoſes.

In the month of Auguſt, and when on the point of undergoing its metamorphoſis, it ſtrips off its ſuperb robe, and puts Time and manner of its change. on

on one of an admirable ſea-green, this ſundamental colour reflects all its various ſhades, according to the different undulations of the animal, and the different accidents of light.

The Sting of the Ichneumon Fly.

Immediate depoſition of thouſands of eggs in its body.

This new decoration is the ſignal of its tortures. Immediately a ſwarm of ichneumon flies aſſail it. I think I am not miſtaken when I aſſert that there is not one of its pores that has not one of thoſe flies faſtened to it. There is even no neceſſity of making uſe of the microſcope to ſee that he is covered with them.

In vain he ſtruggles with all his might, raiſes himſelf upright to get rid of his cruel tormentors—He muſt ſubmit. Thoſe flies, of the ſmalleſt ſpecies, and which can only be ſtudied by means of the microſcope, drive their ſtings into the ſkin of their victim, over the whole extent of his back and ſides. Afterwards, and all at the ſame time, they ſlip their eggs into the bottom of the wounds which they have made.

After having performed this dreadful operation, the ichneumon flies diſappear, and the patient remains for an hour, in a drowzy and even motionleſs ſtate, out of which he awakens to feed with his former voracity. Then he appears much larger, and his ſize increaſes every day. His green colour aſſumes a deeper hue, and the tints produced by the reflection of the light are more ſtrongly marked. The animal in this ſtate of factitious pregnancy, if I may ſo expreſs myſelf, is worthy of all the attention of the obſerver of nature.

I ſhall not undertake the deſcription of the ichneumon fly, it is well deſcribed in the books. If I have obſerved a difference, it is the ſame which exiſts between the European *gnat* and the *muſquitoe* of hot regions, that is to ſay, that our Weſt-India flies are of a leſſer ſize.

I have now to deſcribe the operation which the ichneumon flies, which are extremely ſmall, perform at the very moment of their coming into the world; you will judge, gentlemen, whether this expreſſion is accurate.

Animal Cotton.

Fibrous productions called animal cotton.

A fortnight after the ichneumon flies have thus cruelly depoſited their eggs by perforating the unfortunate caſſada-worm, that

that is to ſay, ſome time in the month of Auguſt, thoſe eggs may be ſeen by the help of a microſcope, hatching on the body of that animal.

Thoſe eggs are all hatched at the ſame moment, and it is impoſſible to catch the moral point of time which may intervene between the birth of one and that of another. At one glance, the caſſada-worm is ſeen covered with all the little worms that have juſt been hatched. They iſſue out of him at every pore, and that *animated robe* covers him ſo entirely, that nothing can be perceived but the top of his head. He then turns to a dirty white, the little worms appear black to the eye, but their true colour is a deep brown.

Hiſtory of the inſects.

This operation laſts hardly more than an hour, and is followed by another which is not much larger but which is much more curious.

As ſoon as the worms are hatched, and without quitting the ſpot where the egg is which they have broke through, they yield a liquid gum, which by coming into contact with the air, becomes ſolid and ſlimy.

At the ſame time, and by a ſimultaneous motion, they raiſe themſelves on their lower extremity, ſhake their heads and one half of their bodies, and ſwing themſelves in every direction. Now is going to begin an operation which will afford the greateſt delight to the admirer of nature.

Each of thoſe *animalculæ* works himſelf a ſmall and almoſt imperceptible cocoon in the ſhape of an egg, in which he wraps himſelf up. Thus, they make, as it were, their winding ſheet. They ſeem to be born but to die.

Thoſe millions and millions of cocoons, all cloſe to each other, and the formation of which has not taken two hours, form a white robe in which the caſſada-worm appears elegantly clothed. While they are thus decking him, he remains in a ſtate of almoſt lethargic torpidity.

As ſoon as this covering is woven, and the little workmen who have made it have retired and hid themſelves in their cells, the worm endeavours to rid himſelf of thoſe barbarous gueſts, and of the robe which contains them, but he does not ſucceed in this attempt without the greateſt efforts.

He comes out of this kind of encloſure, entirely flaccid and dull, inſtead of his former fat and ſhining appearance, his ſkin now

now appears flabby, wrinkled and dirty, and gives him the appearance of decrepitude. He is now an exhausted, suffering being, threatened with approaching death.

He will still gnaw a few leaves, but he no longer eats with that voracious appetite, which indicated an active and vigorous constitution. Shortly afterwards he passes to the state of a chrysalis, and after giving life to thousands of eggs, he suddenly loses his own, leaving to the cultivator who has not yet bethought himself of calculating the advantage that he may draw from him, an advantage which may be so improved as to much more than compensate the ravages which he occasions.

Shell of the Ichneumon Fly.

Shell of the ichneumon fly.

I had imagined that the thousands of little worms which this shell contains in the cocoons of which it is composed, would be hatched some day. I shut it up therefore in a box closed with great caution. Every morning, and very often in the course of the day, I examined it, in order to catch the moment when those little animals were to be born a second time.

In fact, at the expiration of about eight days, I found the inside of the box lined with a cloud of little flies. I made myself certain that they issued out of the little cocoon. Several which issued out of them before my eyes, left me no doubt as to the fact.

I then took up some of those flies, and putting them on a pincer, I examined them with a microscope.

They are bold and lively: they have four wings. Their antennæ are long and vibrating, their belly hangs by a very fine thread: there are some that have a tail, and others that do not shew it. Afterwards I satisfied myself that they feed upon small insects that appear to be of the family of *Acarus*. Those indications appeared to me sufficient to be satisfied that they belong to the family of the ichneumon.

Observations on Animal Cotton.

The animal cotton is white and pure;

I have often held in my hand that cotton shell or wrapper. Its whiteness is dazzling. As soon as the flies have quitted the cocoon, it may be used without any preparatory precaution. It is made up of the purest and finest cotton.

I call

I call it *cotton* becaufe it is *idio-electric* and is pervious to the electric fluid. conducts electricity;

I add to this denomination the epithet *animal*, in contradiftinction to common cotton, which may henceforth be called *vegetable cotton*, fo that the two fpecies may be diftinguifhed from each other by their names, as they are by their origin, although they are very nearly related to each other in their effects.

It is to be obferved, that what might be called *cob-web* in the covering of the fly-carrier, or fmall flocks of filk which are probably intended to fhelter the animal from the rain, is far fuperior to what is called *ferrit* before, and *fleet filk* after the preparation of the finer filk. There is no refufe, no inferior quality in animal-cotton. Every thing in it is as fine and beautiful as can be imagined. the whole is ufeful.

It is poffible, if we may form a judgment by analogy, that medicine, which has extracted from filk what is called *Englifh drops*, a remedy to which the greateft efficacy is attributed, may derive a fimilar advantage, perhaps for the cure of other diforders, from an extract of the animal cotton, which might be called the *St. Domingo drops*.

In fhort there is no need here of any of the precautions which the filk-worm requires. The robe which covers the fly-carrier, is worked every where, and every where perfectly well.

I fhall only obferve that as the rain fpeedily deftroys the caffada-worm, the inftant might be feized on when the ichneumon fly has depofited her eggs, to put the fly-carrier under fhelter. His natural food might be procured for him, as is done with the filk-worm. Caution againft rain.

The ichneumon fly never fails thus to come and depofite her eggs. I have never feen a fly-carrier that was not covered with the robe or fhell that I have fpoken of. I have continued this obfervation for many years, and the crop was fo abundant, that I alone, could collect in lefs than two hours, the quantity of one hundred pints, French meafure.

I repeat it, animal cotton is attended with none of the difficulties which occur in the preparation of vegetable cotton. It is fo pure, that as foon as the ichneumons have left it, which happens eight or ten days after their reclufion, it may be carded and fpun. Animal cotton judged to be much fuperior to the vegetable.

If

If it ſhould want any preparation, it could be only in caſe it ſhould not have been ſufficiently guarded againſt duſt and rain.

Vegetable cotton, beſides the ſeeds that produce it and with which it is charged, is filled with extraneous matter, of which it cannot be freed, but with a minute attention, many hands and much time, or with the help of machines which have not yet been brought to perfection.

In every point of view, animal cotton appears to me to have a great ſuperiority over that of the vegetable kind.

It will, perhaps, be wondered at, that experience has not long ago aſcertained this fact, but let it be conſidered that the ſilk-worm and its uſe, were known long before any uſe was made of them, and that we are now carefully repairing the loſſes that we have ſuffered by the careleſs indifference of our fore-fathers.

The fly-carrier may experience the ſame fate, becauſe it is leſs difficult to reaſon than to make experiments, but I dare hope that as ſoon as it ſhall have prevailed over the ſophiſtry of indolence, it will ſtand the competition with ſilk and vegetable cotton. It is more abundant than either. It requires leſs time and leſs trouble to procure it.

This cotton makes good lint.

I have but one word more to add. Silk and vegetable cotton ſerve only to envenom and inflame wounds, which is attributed to the aſperities of their filaments; I have frequently employed animal cotton as lint in the hoſpital of my plantation, it has always ſupplied the want of that made of flaxen linen, and I have not obſerved the ſmalleſt inconvenience to ariſe from the uſe that I have made of it.

Had it not been for the troubles that have laid our colony waſte, and which have prevented the neceſſary communication, I ſhould have brought to you a fly-carrier in every one of the periods of his life. You would have ſeen the eggs, the magnificent robe with which he is decked at his birth, the kind of food that he is fond of, the ſimple but noble veſtment in which he wraps himſelf up on the approach of his tormentors, you would have ſeen thoſe covering his whole body as it were with points, you would have ſeen him covered with his ſhell, and that ſame ſhell carded, ſpun and ready for the weaver. I had in a great degree already executed this deſign.

But

But it is too well known that I have not been able to ſave any thing in my flight from home, you will, however, be able at a future day to aſcertain the truth of the fact that I have ſtated to you. I thought that a fact of this nature deſerved to be depoſited among your archives, and I may perhaps requeſt of you the permiſſion of depoſiting there ſome other ſtill more curious facts.

BY. DES LOZIERES.

Philadelphia, Feb. 3, 1797.

XV.

An Eſſay on the Fecula of Green Plants. By Profeſſor PROUST.

HILAIRE ROUELLE is the firſt who diſcovered a ſubſtance in the green fecula analogous to the gluten of flour. This ſubſtance, ſince that time, has never appeared doubtful, becauſe there are few chemiſts who have not ſeen its true characters. The fecula, of which it is the baſis, in the opinion of Fourcroy, is only an imaginary being, or, at moſt, it has been too ſlightly examined to be admitted in the number of immediate vegetable products; he even proceeds ſo far as to ſuppoſe that the albumen, that animal product, which no one ever before ſuſpected to be contained in plants, is the ſubſtance which ought to be admitted inſtead of the glutinous part of green feculæ.

Opinion of Fourcroy that the fecula of plants contain albumen and not gluten, as Rouelle aſſerted.

Are albumen and gluten found together or ſeparate in the juice of plants? This is the queſtion I have propoſed to ſolve, and I ſhall endeavour to reſolve it in this part of my obſervations on his "Syſtem of Chemical Knowledge."

are both found in plants?

To ſave my readers the trouble of turning to the work, I ſhall copy the paſſage in which the author has collected the facts and arguments upon which he has formed his judgment. This paſſage is ſtill more remarkable for the difference it preſents between his manner of characteriſing other vegetable products, and that of the chemiſts of the preſent day.

"Rouelle the younger, who examined and particularly compared it with animal matters, aſſerts that he has found it in the coloured feculæ, and eſpecially in that which is called the green fecula of plants. But the name of fecula being

Quotation from Fourcroy.

given indifferently to the fibrous matter contained in the juice of plants, and to the ſtarch, has induced chemiſts to conſider the latter as part of the reſidue of ſolid vegetable ſubſtances, and there is reaſon to think that it was only by analogy, or perhaps by certain equivocal properties, that Rouelle believed the green matter contained any of the glutinous ſubſtances; ſubſequent experiments at leaſt, and ſuch as I have many times repeated on coloured feculæ, have not yielded me the confirmation of this aſſertion, nor has any thing been aſcertained with certainty, that the gluten is one of the principles of this latter fecula."

The name of fecula, ſays Fourcroy, being given *indifferently* to the fibrous matter contained in the juice of plants, and to the ſtarch, has induced chemiſts to conſider the latter as part of the reſidue of ſolid vegetable ſubſtances, and there is reaſon, &c.

He accuſes the ancient chemiſts of inaccuracy,

I ſhall firſt notice that this opinion is not correct. For example, the chemiſts of the preſent day will never agree with Fourcroy, that the confuſion ariſing from the improper application of terms, of which later chemiſts have juſtly complained, has, by a neceſſary conſequence, produced inaccuracy in thoſe who have preceded us. Our maſters, it is true, gave bad names to things, but they did not confound them more than we do.

which is not well founded.

Even at the time when every vegetable precipitate was called a fecula, the ſimilarity of terms never miſled them ſo far as to cauſe them to confound the ſtarch with the reſidue of the ſolid parts of plants. Firſt, we are not acquainted with any reſidue of this deſcription, to which chemiſts can reaſonably compare it; and ſecondly, if any of them did take the green fecula for a reſidue, there was not one of them who did not perfectly know the difference between this fecula or reſidue and ſtarch; and as no ſuch confuſion is to be found in their works, the reproach is unjuſt; we need only look into thoſe of Rouelle, Macquer, Baumé, Sage, Parmentier, &c. to be convinced that the term of fecula has never miſled theſe authors into aſſimilations ſo diſcreditable to their judgment, as to place in the ſame rank the green fecula, the reſidue of the ſolid parts, and the ſtarch.

The fecula was never confounded with fibrous matter.

We will now proceed to the green feculæ, and affirm, that in the laboratories in pharmacy, and ſtill leſs in the hands of a chemiſt ſo celebrated for accuracy of obſervation as Rouelle, the

the broken fibres of green plants have never been confounded with that beautiful soft liquid expressed from their leaves, or with that emulsive product which passes pure through the strainer, and which, by its excessive fineness and the brilliancy of its colour, differs so very much from the herbaceous filaments.

Its state of diffusion in water is not from grinding.

Again, if it were true that the fecula were a body homogeneous with the rest of the plant; if it were possible to consider it as in no respect different from the other parts, but by being more bruised, would it not be possible, by compleating the trituration of the remainder, to convert it also into fecula? When a fresh plant is bruised by the pestle, it is broken small; its texture is destroyed, but it is not pulverized.

This crushing for a few moments, differs too much from a dry pulverization, to admit of any comparison between the fecula so produced, and a moistened powder. When an aqueous juicy plant, a sedum for example, is crushed with a roller on the slab, its expressed juice will afford fecula. Certainly it is not to the trituration, that a fecula is indebted for its softness, its fineness, and the impalpability which distinguishes it from powders. It is molecular in its own nature, and is even, perhaps, crystallized in those fibrous cavities where it is deposited by vegetation.

The doctrines of Rouelle were not mere assertions, but clear investigations from numerous facts.

Rouelle asserts, according to Fourcroy, that the feculæ contains a principle similar to animal matters, &c. Rouelle does more than that: little contented with simple assertions, he proves it, not by *analogy* or *equivocal properties*, but by a succession of convincing facts, by approximations which have been universally admitted, because they combine together the most prominent characters which were then known, or are even yet known, to exist in animal substances. Whence otherwise could Rouelle have drawn his analogies, to enable him to compare, as he does, the green fecula with the gluten of wheat? In fact, what is there in the common appearance of these two products that can lead to the comparison? Their points of comparison must be sought for in their composition, in their chemical properties, and this was done by that indefatigable chemist. These are approximations drawn from analysis which serve as the basis of the Memoir he has written on the green feculæ, and of which there is no mention in the System of Chemical Knowledge; doubtless because in the opinion

which Fourcroy has not detailed.

opinion of its illuftrious author, Rouelle had confounded albumen with the gluten, and the detail of his fuppofed miftake was confidered as a matter of indifference in the hiftory of chemiftry.

Rouelle firft difcovered the peculiar animalized fubftance of the green fecula,

Rouelle however found in the fecula of forrel, a product fo amply poffeffing the chemical properties of albumen, that he particularly infifted upon it in order to fix the attention of his time upon a fubftance fo animalized; and as he afterwards obtained it from a plant, which, according to Fourcroy, does not yield the flighteft trace of albumen, it is now inconteftable, as it was then, that Rouelle was the firft who difcovered in the juices and green feculæ a *product* which, if not intitled to the name of albumen, poffeffes, neverthelefs fo ftrongly, all the properties by which the attention of chemifts has been called exclufively to it, that it is no lefs proper to be urged in the hiftory of their difcoveries, than albumen itfelf.

and the identity of veg. gluten and cheefe.

It is to the fame penetrating eye, the fame impulfe of genius which led him to anticipate difcovery, that he is indebted for that of the aftonifhing refemblance between cafeum and gluten, when they have both undergone that fpecies of fermentation which transforms them into the cellular combination, the odorous and favory compound, called cheefe. And the gluten, in this fingular refult, refembles the other more completely the more carefully it has been wafhed. Macquer, when he publifhed what continues to be every where repeated, that part of thefe changes were occafioned by fome remains of ftarch, was incorrect in his notion. Starch, a fubftance always inactive in fermentation, in that of bread, of beer, and even in germination, could only retard the effect produced by the gluten itfelf, and confequently could only deftroy, in part, thofe characters, from which Rouelle ftated the refemblance of thefe two products.

Peculiar fermentation of cheefe.

And even their analyfis extends far beyond the limits affigned to them; for when the gluten has changed its infipid and vifcous mucofity for the cheefy ftate; when it has gone through all the ftages of that fermentation which is effential to that condition, it is found alfo to have acquired the tafte of thofe fharp and burning falts which conftitute the principal merit of the Roquefort cheefe; falts which have nothing in common with what is added, but are found equally powerful in the curd which has been wafhed and left to its own fermentation.

3 In

In fact in the cheese from gluten, as well as in that from animals, potash and sulphuric acid enable us to find that ammonia, and that vinegar discovered by Vauquelin. Is the ammoniacal acetite then one of the ingredients which gives the flavour to cheese? I only know that alcohol applied to strong cheese deprives it of all its taste. An analysis of that species might afford curious results; but let us return to the green feculæ; examine them by the lights of modern chemistry, and endeavour more particularly to discover whether albumen actually exists where Beccari and Rouelle found gluten.

Salts generated in cheese, &c.

Green Fecula.

I. The fecula undergoes, by heat, a change capable alone of giving it a decisive character as to its nature. I mean that concrescibility which has so few examples in vegetable products; that agglutination which compresses the particles together, and produces the appearance of a cheesy curd. Though the fecula, before this change, passed easily through the strainer, it can no longer do so after having been heated; a peculiar hardening has deprived it of its tenuity; but heat does not coagulate the fibrous tissue; in this respect the fecula does not resemble the broken straw of green plants.

The green fecula very concrescible by heat.

II. The fecula, separated from the juice by filtration, acquires an elastic and horny consistence by drying. It is softened with difficulty in heated water, but will not become soft even at the end of a month; notwithstanding it is moistened it still retains its horny state. When bended it will return to its shape, and cannot by any means be crumbled: these qualities also are not found in the dry woody pulp.

Elastic and horny when dry.

The feculæ of green and white cabbages, cresses, hemlock, &c. do not lose the property of coagulating by heat from that cause. Into water heated to between 50 and 60 degrees, plunge two matrasses of equal size, one containing diluted fecula, and the other with the water and white of an egg; the fecula will harden and be collected in flocks, such as are formed in a juice which is clarified by heat; but at this temperature the albumen will not even lose its transparency.

More concrescible than white of egg;

III. The green fecula is very nearly in equipoise with water, for that of plants which are not acid, frequently requires a week to precipitate.

Put

precipitable from water by alcohol and by acids.

Put some fecula washed and diluted into three glasses of equal size. To the first add a little alcohol, a few drops of acid to the second, and place the third between them for comparison. In the two first the precipitation will be compleat in half an hour, while in the third it will be hardly begun; alcohol and the acids therefore coagulate feculæ; but they have no such action upon the woody residue.

Alcohol takes up near one sixth of resinous matter.

IV. One hundred parts of the dried fecula of hemlock yielded to alcohol from 15 to 16 of green resin. After repeated infusions to which it was subjected, it remained of an earthy grey, and the alcohol was incapable of bleaching it. Sage, who was well acquainted with feculæ, found that they yielded about one third of their weight in resin; in order to separate it easily, the fecula must be thrown into spirits of wine while yet moist, the fluid then penetrates and attacks all its parts; but the effect is much more difficult when it has become horny by drying.

(To be continued.)

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Prize Questions of Foreign Learned Societies.

National Institute. Prize questions.

THE National Institute of France held a public sitting on the 20th Vendemiaire, when the new subjects for prizes were announced.

The *Class of Mathematical and Physical Knowledge* proposed the following question:

Winter sleep of animals.

" To determine, by anatomical and chemical observations and experiments, what are the phenomena of inactivity which certain animals, such as marmots, dormice, &c. undergo in the winter, with regard to the circulation of the blood, the respiration and the irritability; to inquire what are the causes of their sleeping, and why it is peculiar to these animals."

This question was proposed before, and the prize was to have been decreed in this sitting; but the meeting being of opinion that the memoirs received did not contain sufficient information, decreed that it should be proposed again, and that the prize should be doubled. It will be of the value of two kilogrammes of gold (about 6.800 fr.); and will be distributed

tributed at the public fitting of Vendemiaire in the year 13. Memoirs muft be tranfmitted to the Secretary of the Inftitute before the 15th Meffidor of the year 12.

The queftion propofed by the *Clafs of Moral and Political* knowledge is,

"To determine how the faculty of thought can be decompofed; and what are the elementary faculties which can be difcovered in it?" Decompofition of thought.

The prize is a gold metal of the weight of five hectogrammes (about 1700 fr.): it will be adjudged at the public fitting of Germinal in the 12th year of the Republic.

The works cannot be received after the 15th Nivofe of the fame year.

Prize for Geography. "To compare the geographical charts by Ptolemy, of the interior of Africa, with thofe which have been tranfmitted to us by fubfequent geographers and hiftorians, with the exception of Egypt and the coafts of Barbary from Tunis to Morocco." Geography prize.

This fubject had been propofed in the year 9, but the memoirs fent not correfponding to the conditions of the notice, it was renewed.

The prize is a gold medal weighing five hectogrammes (about 1700 fr.), and will be adjudged in the public fitting of Meffidor in the year 12. The memoirs muft be fent in before the 15th Germinal of the fame year.

The Medical Society at Montpellier has propofed two prizes of 500 francs each, of which the firft is to be adjudged in May 1803, and the fecond in May 1804, on the following queftions: Med. Soc. of Montpellier. Prize queftions.

1ft. In what kinds of difeafes, and under what circumftances, is inflammation favourable or dangerous? and, in the treatment of fuch difeafes, in what cafes ought it to be excited or checked. Inflammation?

2d. To afcertain by experiment and obfervation, what degree of confidence can be placed on the ufe of certain fubftances, by friction, which are generally adminiftered internally; to determine the effect of fuch remedies, in both methods, and alfo the quantity of the dofe; to point out the difeafes and cafes in which one method is to be preferred to the other; and finally, to determine, in the different maladies, to what parts of the body the application of thefe remedies, are moft efficacious. Medicines by friction?

Flafh

Flaſh from an Air-Gun.

Luminous flaſh from an air gun.

M. Pictet, in a letter from Paris of the 1ſt of January laſt to Mr. Tilloch (Philoſ. Magazine, Vol. XIV. 363.) ſtates a communication to the National Inſtitute of France on the 29th of December by M. Mollet, of Lyons, reſpecting the luminous appearance produced by the diſcharge of an air-gun in the dark, a phenomenon which he conſiders as never having been before obſerved. It has, however, been known for ſome time in this country, having been firſt mentioned, I believe, near a year and a half ago by Mr. Fletcher at a meeting for philoſophical experiments and converſations, which was then held weekly at my houſe. Several diſcuſſions accordingly took place with regard to its cauſe,—as, whether it was produced by electricity, or the change of capacity in the expanding fluid, &c. and it was intended to inſtitute a courſe of experiments on the ſubject, but ſome other objects of enquiry intervening the matter was poſtponed. It is a curious phenomenon, and deſerves inveſtigation.

Letter from Profeſſor PROUST *to J. C.* DELAMETHERIE.

On the Sugar of Grapes, and the Compound Nature of Urée.

New ſugar in the grape.

I have diſcovered a new ſugar in the grape, which is the baſis of wine; it is different from that of the ſugar-cane, cryſtalliſes differently, &c. It is contained in the proportion of about 30 per cent. in the juice of the grape. Azote is uniformly combined with the carbonic acid in the fermentation of wine: in that of gluten it is pure hidrogen, which is diſengaged with the carbonic acid.

The urée is a compound containing ammonia.

Tell Vauquelin that the urée, in the ſtate in which they have examined it, is a ſaline ſubſtance ſaturated with ammonia, and not a ſimple product; he need only apply ſulphuric acid to carry off this ammonia, and to have the urée pure, but coloured with a reſin from which I have not yet been able to free it.

J. de Phyſique.

On

On the Use of Sulphate of Soda in the Manufactory of Glass. By PAJET-DESCHARMES *.

Since the notice which was given in the *Journal de Physique* that sulphate of soda might be used, without preparation or any intermediate agent, in the fabrication of siliceous glass, Cit. Pajot-Descharmes has thought fit to publish the result of his principal experiments on the employment of this salt in glass-making.

Use of sulphate of soda in glass making.

It appears from the experiments which this operator has reported in the *Journal de Physique,*

1. That sulphate of soda and sand alone, in various proportions cannot succeed.

2. That sulphate of soda, mixed with pounded charcoal, in the proportion of a tenth or twentieth part, yielded a yellow glass more or less black, of the nature of obsidian pastes or stones: the crucibles were then very slightly acted upon.

3. That equal parts of carbonate of lime, dried sulphate of soda, and sand, produced a beautiful glass, clear, solid, and of a pale yellow: the crucibles were then very little corroded.

Cit. Pajot-Descharmes observes, that notwithstanding the pains he took, he could never obtain a glass with sulphate of soda that was not of a yellowish green, whereas muriate of soda (sea-salt), treated in the same manner as sulphate of soda, always produced a glass of a light blue tinge inclining more or less to green.

Cit. Pajot-Descharmes proposes to treat more fully, in a particular memoir, on all the experiments he has made on this subject.

Observations on the Necessity of immersing Seeds in Water in Times of Drought. By Ant. ALEXIS CADET-DE-VAUX. †

Franconville-la-Garenne, 2 Brumaire (Oct. 23.)

To reap, we must sow; and, from the drought which has prevailed for six months, sowing is not easy, for our gardens are

In dry seasons the seed grain is in danger of perishing in the ground.

* Abridged with remarks in the Journal des Mines, No. 69. from a Letter from Cit. Pajot-Descharmes, to J. C. de la Metherie, in the Journ. de Physique, Vol. LII. p. 210.

† Decade Philosophique, No. 4. An. XI.

are no longer cultivated with the ſpade and the harrow, but with the mattock and the pick-axe; the plough, however, on account of its ſtrength, can ſtill be employed to till the ground, except in compact lands, and thoſe which are ſtiffly bound. But it is not enough that the land ſhall have been tilled to enable us to ſow it, the ſeed muſt alſo germinate, without which it dries and periſhes, or becomes the food of animals and inſects. For without rain, or dew which moiſtens at leaſt the ſurface of the ſoil, there can be no germination. In the mean time the ſeaſon advances, and the ſeed-time is already late. Let us point out, then, to the huſbandman, a method of preventing the inconvenience of drought; it is, not to commit his ſeed to the earth, until it is impregnated with the moiſture neceſſary for its germination.

Remedy; to ſoak it firſt in water.

We may refer to a great example: the Chineſe do not depoſit a ſingle ſeed in the earth until it has been immerſed in water. And I will adduce an experiment nearer home, which is in favour of this practice. Five years ago, I ſowed half an acre (*arpent*) of land with wild ſuccory, lucern, and pimpernel. With a view to compare the produce of theſe plants, I ſowed them in rows two feet aſunder. The ſpring was very dry, and I ſoaked each of the ſeeds in water for forty eight hours. The quantity ſoaked was not ſufficient; for there was required as much more as to ſow nine rows, three of each kind. I took advantage of this circumſtance to compare the effect ariſing from the ſeed being prepared or not prepared by immerſion. The reſult was, that in the nine rows ſowed with the dry ſeed, not more than thirty plants came up in five months: while the remainder of the land was covered, and formed a moſt beautiful artificial meadow. The rain fell too late to ſow theſe nine rows, and it was neceſſary to ſow them again in autumn.

Experimental proof.

Let us now apply theſe facts to the ſowing of corn.

Liming recommended; by immerſion.

The huſbandman is in the habit of liming his wheat when he apprehends the rot. This year he has no need to dread that evil; nevertheleſs let him uſe the lime, but by immerſion; for in general the method of application is defective. The operation is uſually confined to a ſimple ſprinkling of the heap of wheat with lime water, while it is turned over with a ſhovel.

The

The good, the only way of liming, is by immerſion: put the ſeed into tubs, and cover it to the height of four or five fingers breadths with lime-water, made ſo hot that the hand cannot be kept in it without difficulty; cover it up, ſtirring it three or four times in the twenty-four hours; after which draw out the bung that the water may run off, the quantity will be but ſmall; it will be nearly all abſorbed by the grain, which muſt be taken from the tubs, ſpread out in the air and then ſowed. Method deſcribed.

Twelve buſhels of wheat, immerſed for twenty-four hours, will abſorb nearly one-fourth of water, that is to ſay, they will ſwell to fifteen or ſixteen buſhels by meaſure. Let us now inveſtigate the theory. Every grain of this corn carries with it to the earth a quantity of water, more than ſufficient to enſure its germination. This water acts principally upon the extractive matter of the huſk; it diſſolves this principle, one of the properties of which is to attract and ſtrongly to retain moiſture. Hence this water will not be evaporated. If inſtead of pure water, we uſe dunghill water, which is ſaturated with this extractive matter, together with deliqueſcent ſalts, and fatty matter, then the moſt minute quantities of ſurrounding humidity will be attracted towards the grain. But in truth it will, after this treatment, ſucceed very well without the ſpeedy aſſiſtance of the rains and dews; it poſſeſſes a ſufficiency of moiſture to put forth its germ, to throw out its radical, and in ſhort, to ſecure its germination. The grain which has been ſteeped gains, in ordinary ſeaſons, from twelve to ſixteen days in advance before that which has not been ſteeped; and in times of exceſſive drought, it gains every thing. If ſteeped, it germinates and grows; and if not ſteeped it dries and periſhes. Let the rains come, let them continue, ſtill I adviſe immerſion; which by forwarding the germination, remedies the inconveniencies of a late ſeed-time.

Wheat becomes one fourth larger by abſorbing water.

Dunghill water is beſt.

Recapitulation.

Extract from a Memoir by CIT. FOURCROY, *on the Chemical Nature of Ants, and on the ſimultaneous Exiſtence of two Vegetable Acids in theſe Inſects.*

Samuel Fiſcher firſt deſcribed this acid in 1670. It has ſince been more particularly examined by Margraff, Arvidſon, Bergman, &c. and finally by Cit. Deyeux, who eſtabliſhed the identity, which Margraff had previouſly ſuſpected to

Experiments ſhewing the exiſtence of malic as well as formic acid in ants.

Experiments ſhewing the exiſtence of malic as well as formic acid in ants.

to exiſt between the formic and acetous acids. Nevertheleſs there were ſtill ſome doubts to remove, and it was theſe which induced the Citizens Fourcroy and Vauquelin to undertake the following inveſtigations:

Some red ants (*formica rufa*, Lin.) were cruſhed in a marble mortar. A ſharp vapour was diſengaged, ſimilar to that of radical vinegar; and the alcohol, in which the ants were put to macerate, was tinged yellow.

This infuſion produced, by diſtillation, an inflammable liquor, ſlightly acid. At the ſame time it formed a browniſh ſediment, which was carefully ſeparated. This ſediment became covered with an acid liquor, which was ſaturated with lime.

The latter combination became brown and thick: it had a pungent nauſeous taſte, and the air produced bubbles in it, as in ſoap-ſuds.

One part of this compound, mixed with one part and a half of ſulphuric acid and two of water, produced a very thick magma, which, by diſtillation, yielded an acid liquor, without colour, of an empyreumatic odour, but which did not diſcover any trace of ſulphuric acid.

This acid combined with potaſh, formed a true acetite.

The browniſh thick compound, of which we have ſpoken above, formed, by ſolution in acetite of lead, an abundant precipitate, which proves that the acid extracted from the ants by the alcohol, contained ſomething beſide the acetous acid.

The ſame calcareous compound mixed with a ſolution of nitrate of lead, yielded an abundant yellow precipitate, which, treated with ſulphuric acid weakened by water, formed a new precipitate, heavier and whiter. The ſupernatant fluid was ſlightly acid and ſugary: it precipitated abundantly the nitrates of mercury, of ſilver, and of lead.

Many other facts, in addition to thoſe we have mentioned, are ſufficient proofs that the malic acid is joined with acetic acid in the liquor which is extracted from the ants by the alcohol; and it is doubtleſs the preſence of this acid which has led thoſe chemiſts into an error, who formerly treated of this ſubject.

The ants, after having been ſeparated from the alcohol, yielded, by diſtillation, an empyreumatic ſetid oil, carbonate of

of ammonia and acetite of ammonia, the whole diſſolved in a great quantity of water.

The brown ſubſtance left from the diſtillation of the infuſion in alcohol, was inſoluble in water, and ſoluble in alcohol, except a ſmall quantity of a browniſh matter, which appeared to the authors to be albumen. The ſolution of this brown ſubſtance in alcohol became milky by the addition of water; and, in a few days, depoſited a precipitate of a reſinous appearance, which ſeemed to be a fat matter of a peculiar nature.

Laſtly, the reſiduum from the ants was an animal carbon, which, after combuſtion, left only phoſphate of lime.

The memoir finiſhes with obſervations on the preſence of the acetous and malic acids in ants in particular, and in organic bodies in general.

Bulletin des Sciences, No. 70.

An Eſſay on the Relation between the ſpecific Gravities and the Strengths and Values of Spirituous Liquors; with Rules for the Adaptation of Mr. Gilpin's Tables to the preſent Standard, and two New Tables for finding the Per-centage and Concentration when the ſpecific Gravity and Temperature are given. By ATKINS and Co. *Mathematical Inſtrument Makers. Quarto, 74 Pages. London, Cadell and Davies, and Robinſons,* 1803.

Atkins on the ſpecific gravities of ſpirituous liquors.

It is certainly very remarkable, that no mode of denomination by which the real ſtrength of ſpirituous liquors could be defined with any tolerable degree of correctneſs, has ever yet been in general uſe in this or any other country. The miſchiefs accruing from the want of ſuch a ſyſtem, and the diſputes which were daily ariſing between the revenue officers and the traders, with reſpect to theſe matters, induced the legiſlature ſeveral years ſince to paſs an act by which was declared, that all ſpirits ſhould be deemed and taken to be of the degree of ſtrength which ſhould be denoted by Clarke's hydrometer. The inconveniences, however, which reſulted both from the complex conſtruction of the inſtrument itſelf, and the ineligibility of the mode by which the ſtrengths of theſe liquors are denominated, according to the ſyſtem propoſed by its inventor, having been long felt, and both one

and

Atkins on the ſpecific gravities of ſpirituous liquors.

and the other having accordingly in a great meaſure fallen into diſuſe amongſt all but thoſe immediately concerned in the collection of the duties, the lords of the treaſury during the laſt ſeſſion of parliament, applied for and obtained an act, empowering them to order any other hydrometer to be uſed for the purpoſes of the revenue, which might be found more conveniently applicable to them. This has of courſe rendered a review of the whole matter indiſpenſably neceſſary, and the authors of the pamphlet before us, whoſe attention to this ſubject is already ſufficiently known, have accordingly with great perſpicuity traced the principles on which alone an equitable ſyſtem can be eſtabliſhed.

After ſhewing the connection between the ſubject of the preſent tract, and the appreciation of our weights and meaſures in general, they proceed in the preface to ſtate the authorities on which they have founded their eſtimation of the weights of the known meaſures of diſtilled water. Theſe are principally the experiments of Sir George Shuckburgh Evelyn *, and thoſe of the French commiſſioners of weights and meaſures. From the former it appears to reſult, that the cubic inch of diſtilled water at 60° of Fahrenheit's thermometer, weighed in air at the ſame temperature when the barometer ſtands at 29½ inches, is equal to 252.506 troy grains, and from the latter, equal to 252.55, ſo that the authors take it to be under the circumſtances ſufficiently near 252½, and that this weight therefore correſponds with the ſpecific gravity of 1,000. Their appreciation of the cubic meaſure of the wine gallon is ſimilar to that of the board of exciſe, viz. that it contains 231. cubic inches, and they eſtimate the pound avoirdupoiſe at 7,000 grains troy. The preliminaries and a detail of the calculations and reaſons on which the deductions are founded, are very properly introduced in this place, as neceſſary to the underſtanding of their ſubſequent eſtimation of the ſpecific gravity of proof ſpirit.

The work itſelf treats of the ſubject under the following general heads; of the general relation between the ſpecific gravities and the ſtrengths and values of ſpirituous liquors, and the circumſtances by which the former are influenced; of the

* See Phil. Journal, Quarto Series, Vol. iii. p. 97, &c. and Octavo Series, Vol. iv. p. 35. (No. for January laſt.)

ſtandard of proof; of over-proofs and under-proofs, and the modes of appreciating them; of Mr. Gilpin's experiments and tables; and of the means of adapting them to the preſent ſtandard, with various problems and rules for that purpoſe; and concludes with the two tables of the authors mentioned in the title-page.

Atkins on the ſpecific gravities of ſpirituous liquors.

The firſt chapter treats very perſpicuouſly of the ſubject in general, and the intricacy in which it is involved by the joint effects of concentration and change of temperature.

In the ſecond the authors proceed to the eſtimation of the ſtrength of proof ſpirit, as deducible from a looſe paſſage in an act of parliament paſſed in 1762, by which it is enacted, that "for the purpoſes of that act, each gallon of brandy or ſpirits of the ſtrength of one to ſix under hydrometer-proof, ſhall be taken and reckoned at 7 lbs. 13 oz. the gallon; and which ſeems to be the only clauſe in the ſtatute-book, in which the ſtrength of any kind of ſpirituous liquor is attempted to be defined by reference to its ſpecific gravity. The temperature of the liquor and ſome other circumſtances are here left to be aſſumed, which the authors have accordingly been under the neceſſity of doing, and from hence they deduce, that proof ſpirit may reaſonably be conſidered as having the ſpecific gravity of 920 at 60°. according to the common acceptation; and this they accordingly recommend as the future legal definition of that ſtandard.

The third chapter contains a diſtinct account of the various modes of compariſon hitherto in uſe, and of their reſpective inconveniences and defects. According to the ſyſtem propoſed by the authors, the denomination itſelf would at once indicate the real comparative value of the ſpirit in queſtion, by reference to the equivalent quantity of proof ſpirit, or that which would produce or be producible from 100 gallons of the former, by the addition of water to the ſtronger of the two, till they were reduced to the ſame degree of ſtrength; allowance being made in all caſes for the concentration which takes place by mixture, and the change in bulk and conſequent difference in value by meaſure, according to the exiſting temperature. Thus, an under-proof of which 100 gallons might be made up with water from 80 gallons of proof, would be called ſpirit of "80" or "20 under-proof," and a ſpirit of which 100 gallons would when reduced to proof, make 134, would be called "134,

or

Atkins on the ſpecific gravities of ſpirituous liquors.

or "34 over-proof." We here find ſome arithmetical rules introduced for the purpoſe of illuſtrating the practical facilities reſulting from the uſe of ſuch a ſyſtem, and the chapter concludes with a ſection on the general conſtruction of the hydrometer, in which, however, the authors have modeſtly forborne to ſpeak particularly of thoſe of their own conſtruction, which are already ſo well known to the public.

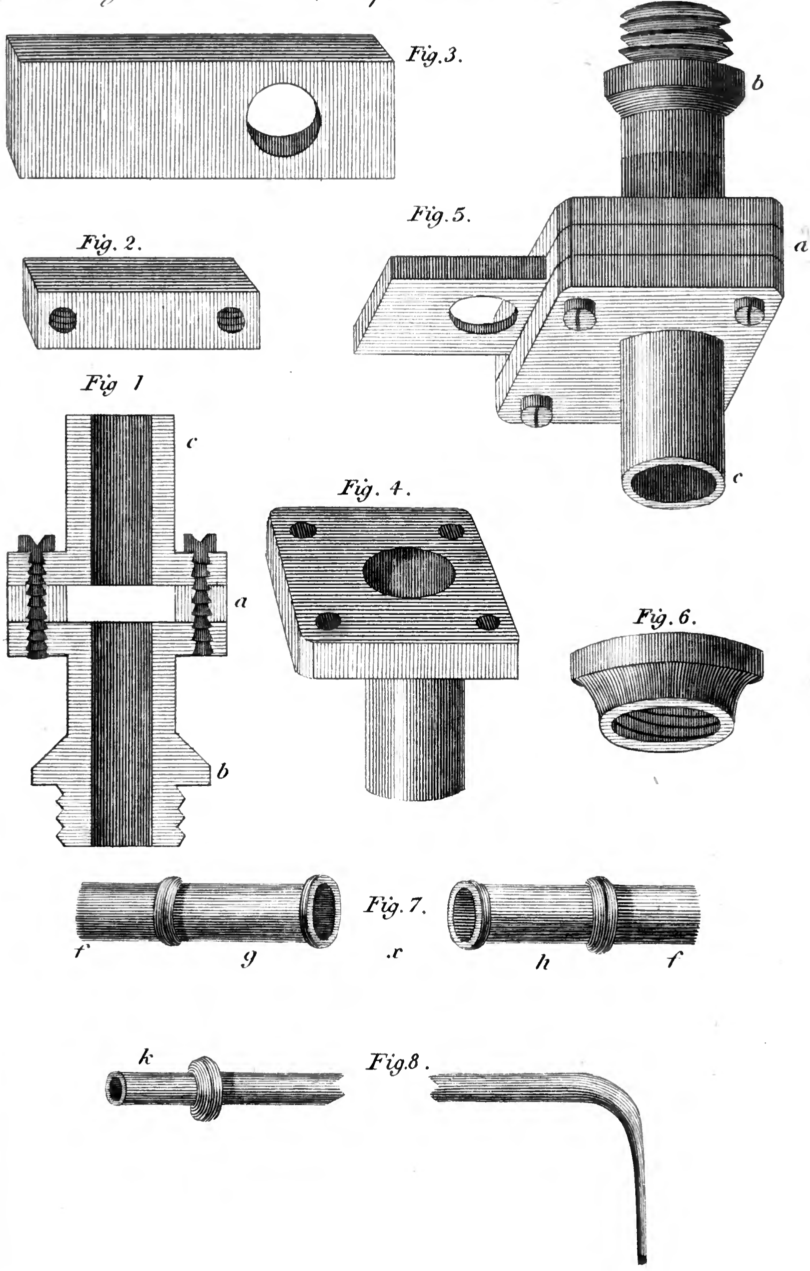
In the fourth chapter, which treats of the experiments and tables of Mr. Gilpin, the author ſpeaks with its merited praiſe of this great work, and in the fifth they give a variety of rules, ſome of them very curious, for ſolving all the moſt uſeful problems reſpecting this ſubject from theſe tables, which they conſider as an authentic regiſter of experimental reſults. Theſe proceſſes are all founded on the ſuppoſition that proof ſpirit is of the ſpecific gravity of 920 at 60°.

The ſixth and laſt chapter contains the two tables of the authors already mentioned, with their uſe and application. Theſe tables in a great meaſure anſwer all the purpoſes of the more voluminous ones of Mr. Gilpin, from which they are calculated by the preceding formulæ, and that in a much ſimpler and eaſier manner, being adapted to the preſent ſtandard of proof, and giving the required per-centage and concentration by mere inſpection.

Having thus ſtated the outlines of this work, it would be to take notice of a circumſtance of minor conſequence if I remarked, that it is a very neat ſpecimen of typographical elegance. The ſubject is a moſt important one, and the philoſophical reader will be pleaſed to find it here treated in a manner which renders it no leſs worthy of the attention of the man of ſcience, than of thoſe whoſe commercial intereſts are involved in the event of the preſent inveſtigation.

A. B.

Sliding Stop-cock for pneumatic Experiments



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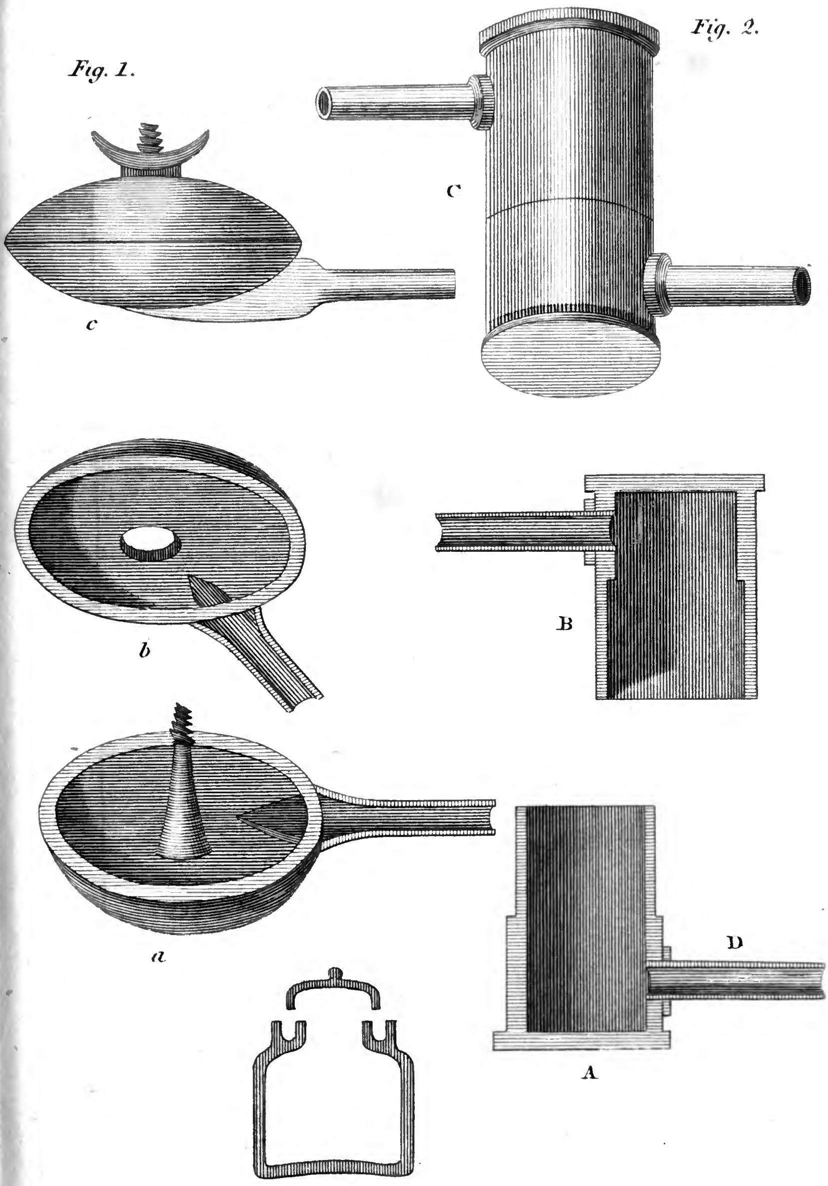
Metallic Joints for pneumatic Apparatus



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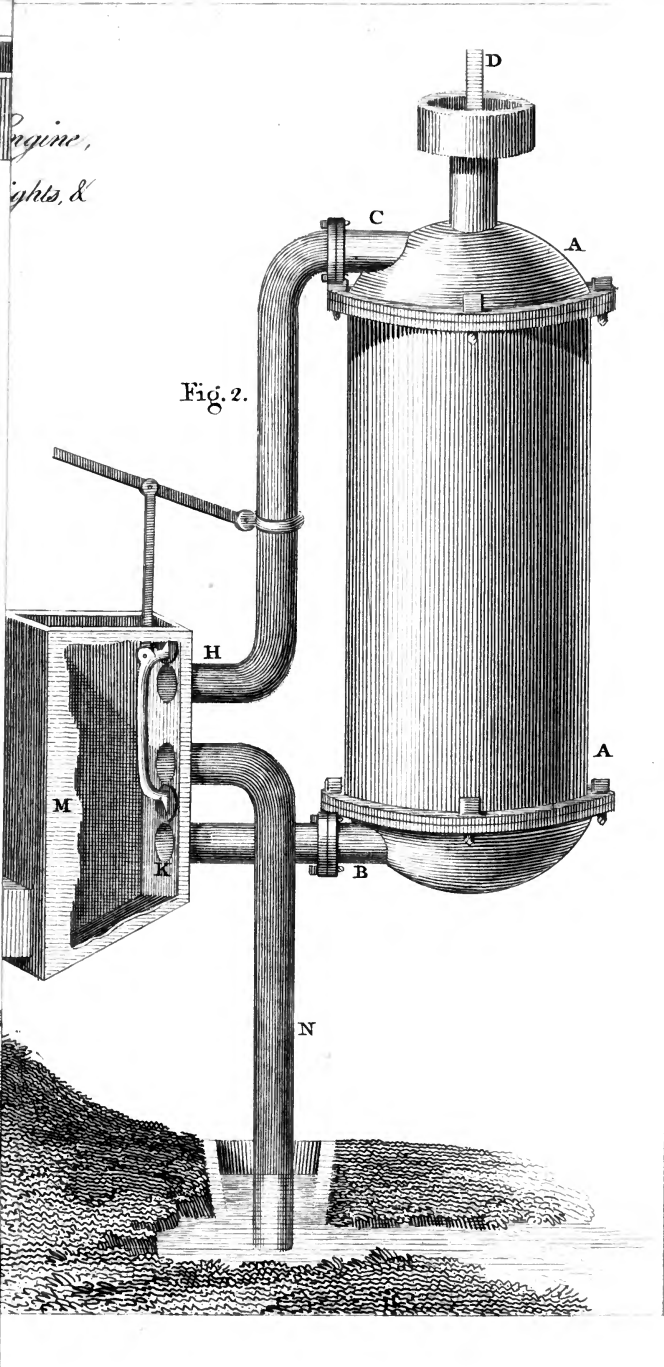
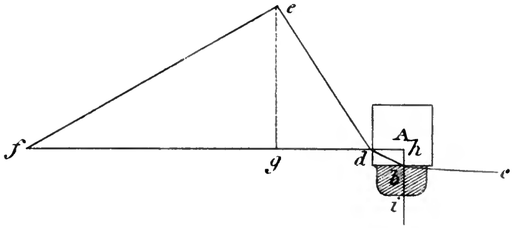


Fig. 2.

NATURAL HISTORY

Dr. Wollaston's Method of examining the refractive & dispersive powers of Bodies.



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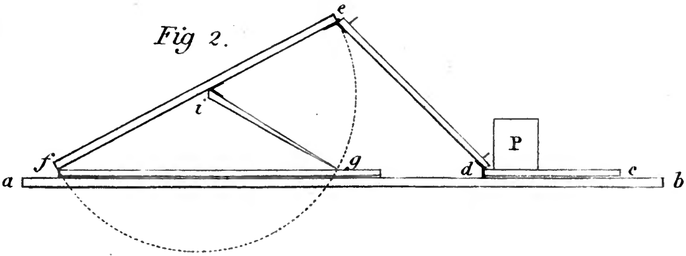

Fig 2.

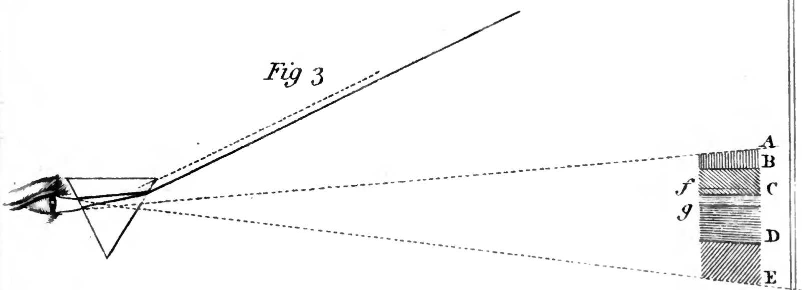

Fig 3

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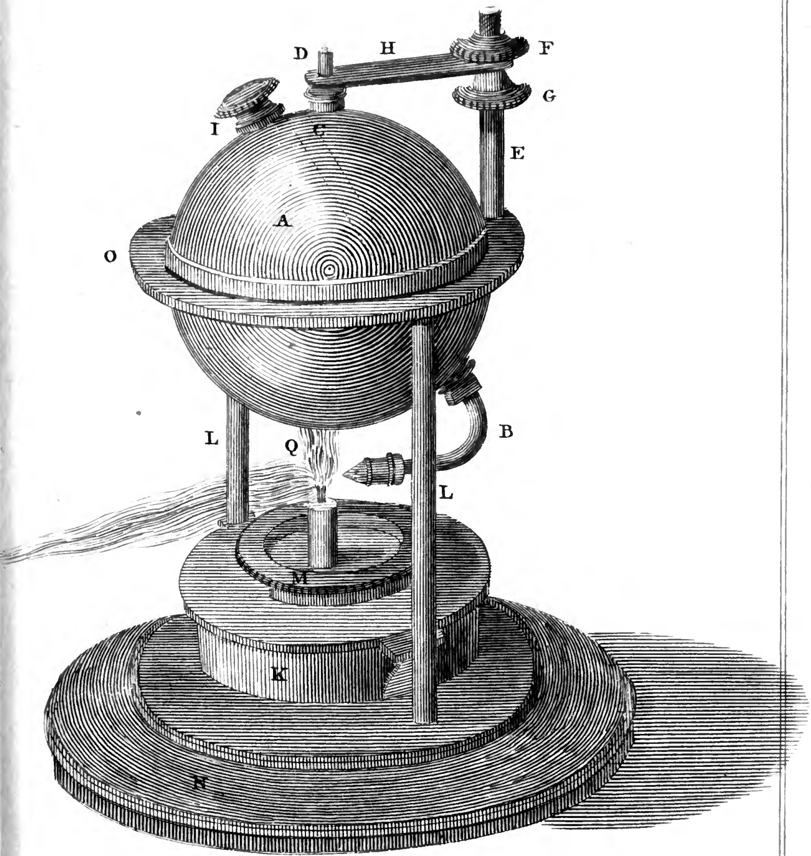
Blow Pipe, by Alcohol. By Mr. Hooke.



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Dr. Young's Harmonic Sliders.

Fig. 1.

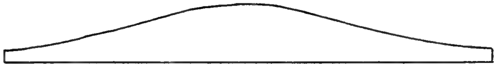


Fig. 2.

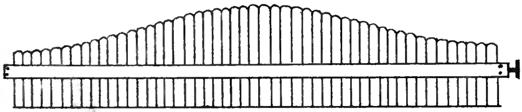
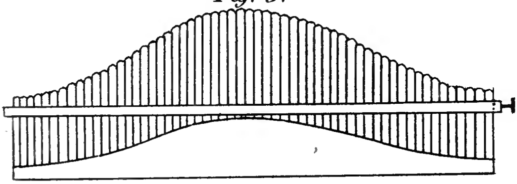


Fig. 3.



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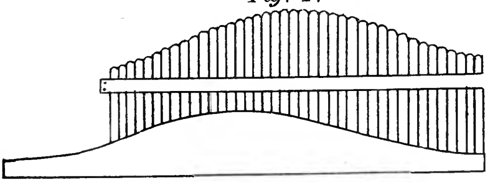

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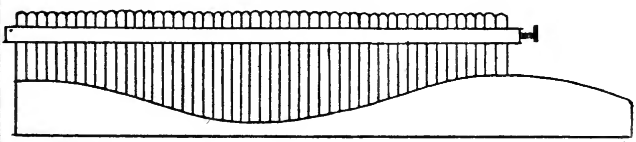

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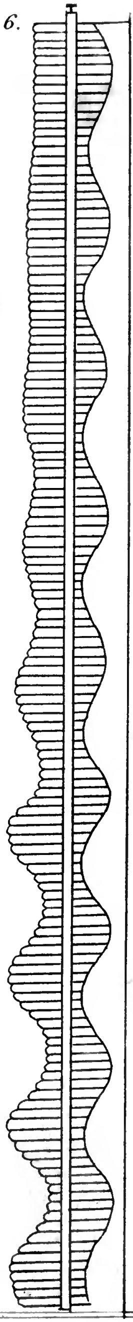

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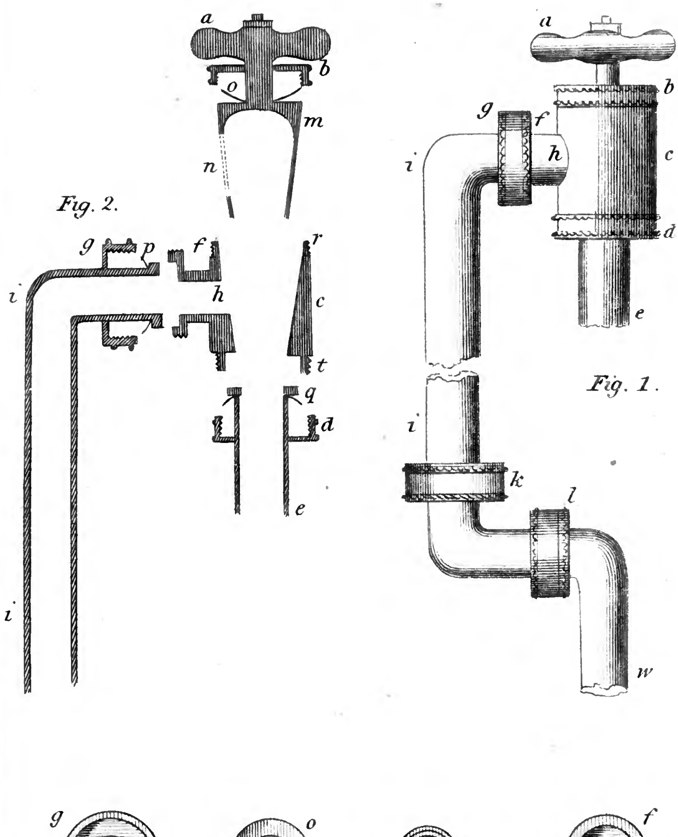
Steam-cocks, Joints & Tubes, W. N.



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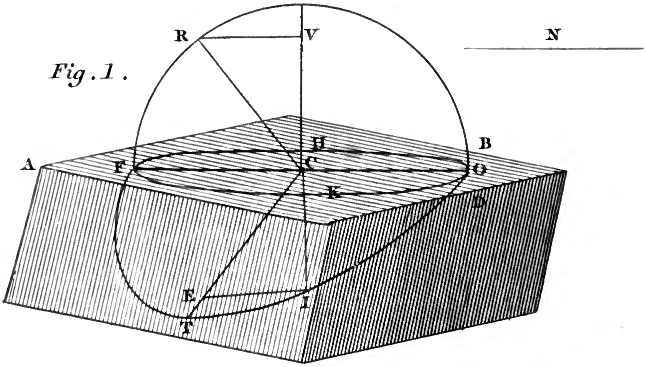
NATURAL HISTORY.

Dr. Wollaston, on the oblique refraction of Iceland Crystal.

Fig. 1.



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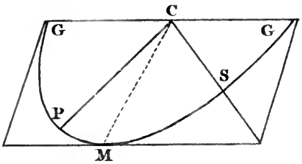

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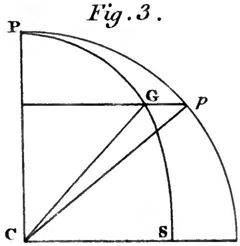


Fig. 4.

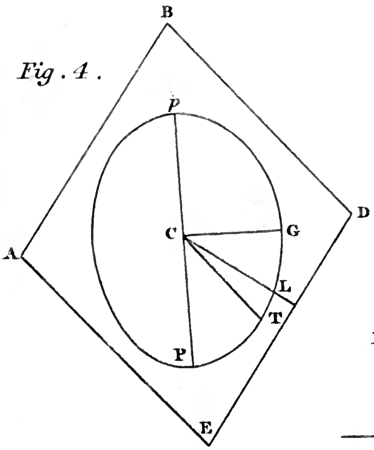
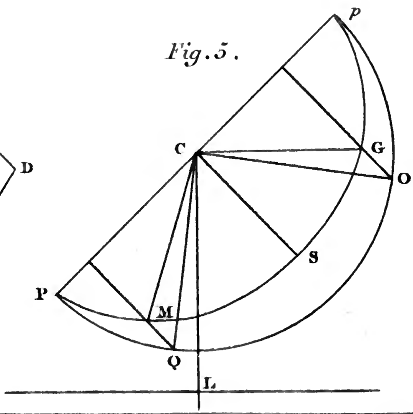


Fig. 5.



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Mr. Dalton's Sketch of the Rivers in England & Wales, divided into districts.

Mr. Gough, on sonorous Undulation.

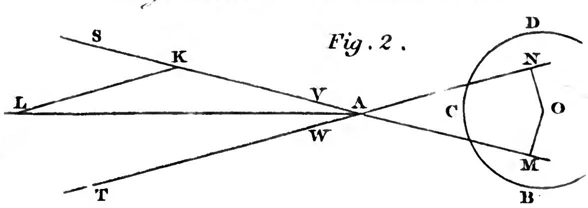

Fig. 2.

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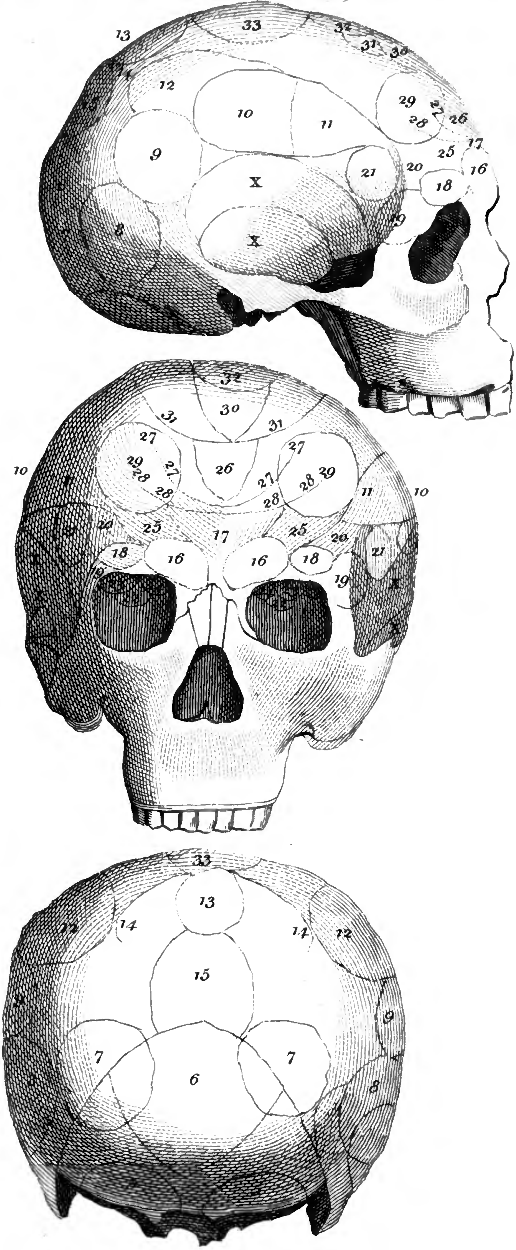
The craniognomic System of Dr. Gall, of Vienna.



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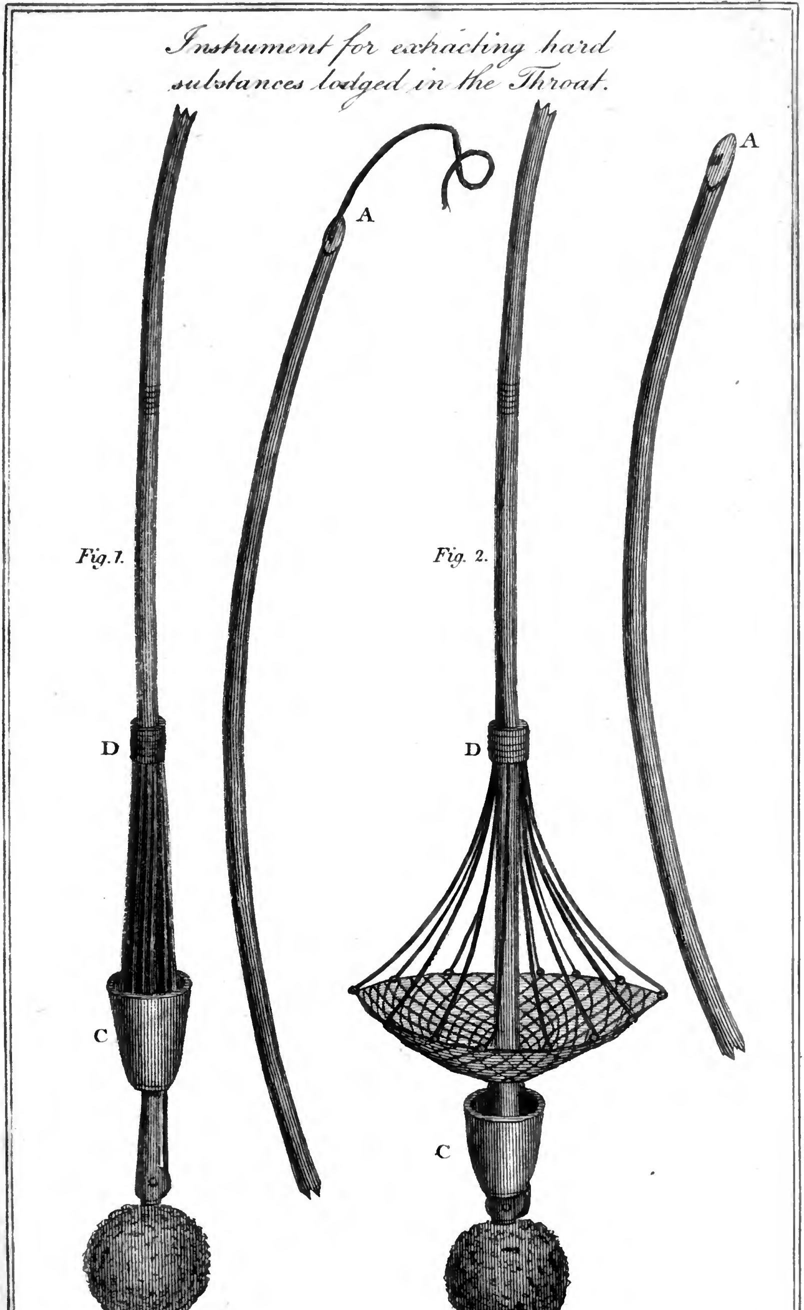

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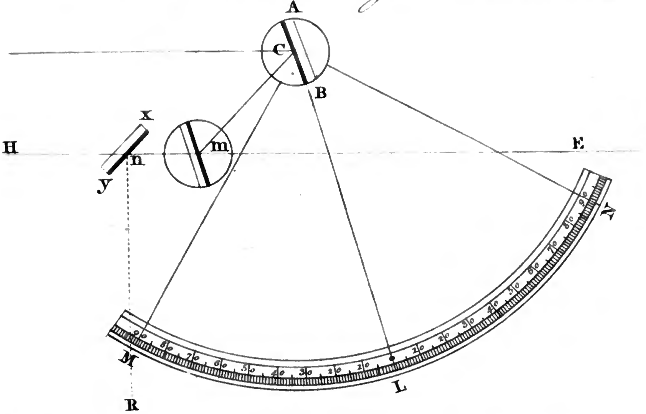
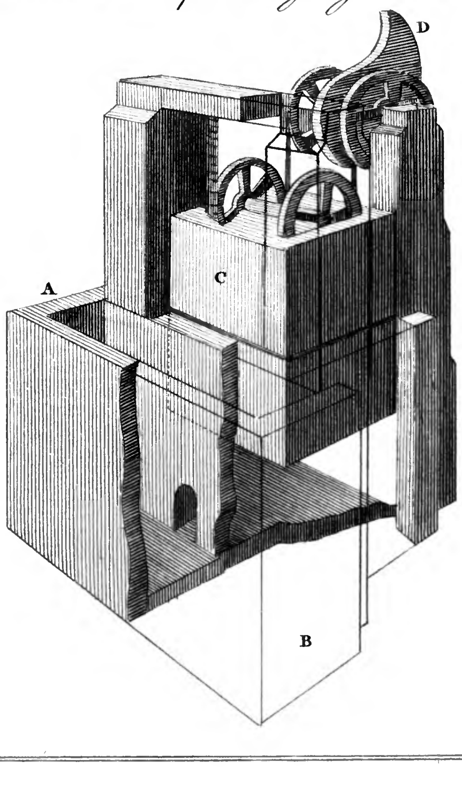
Mr. E. Walker's reflecting Quadrant.



Mr. Hudleston's method of conveying Boats &c.



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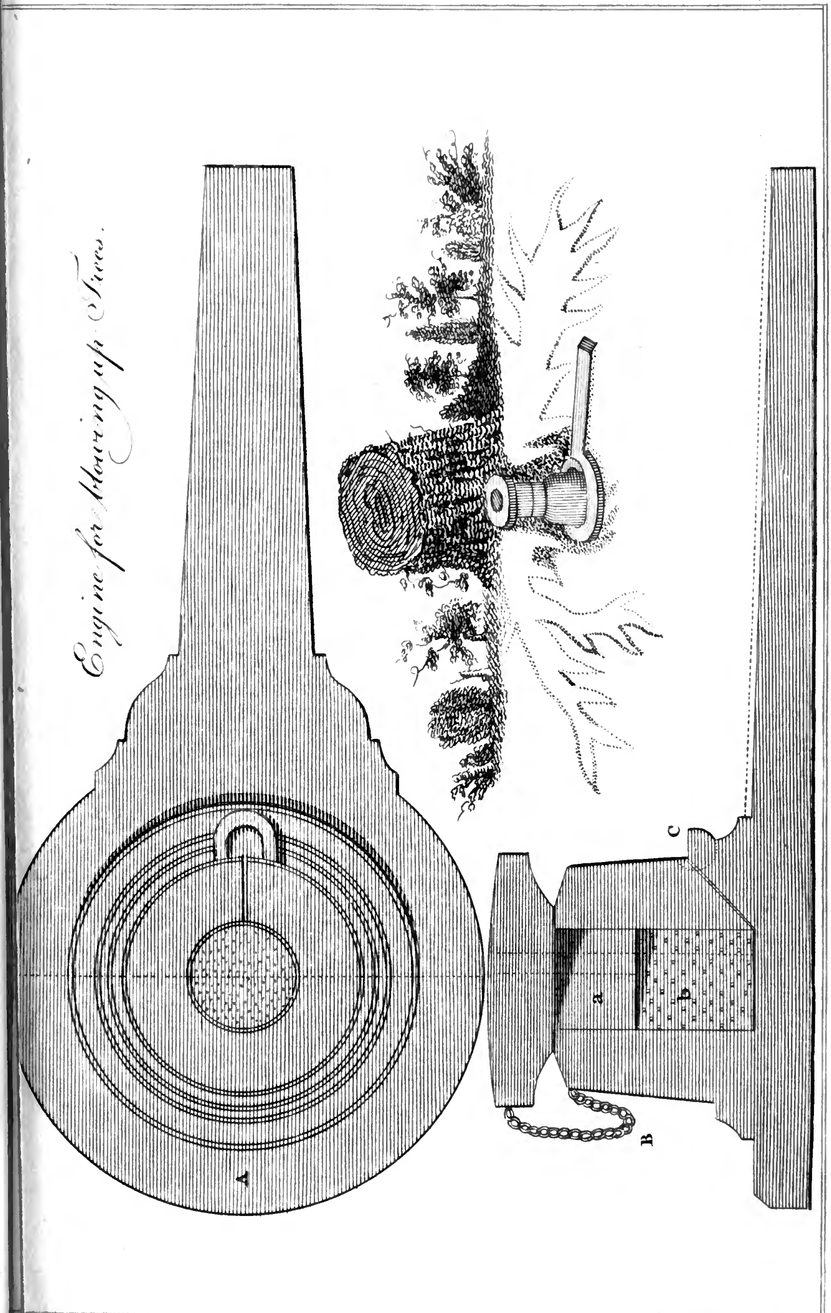

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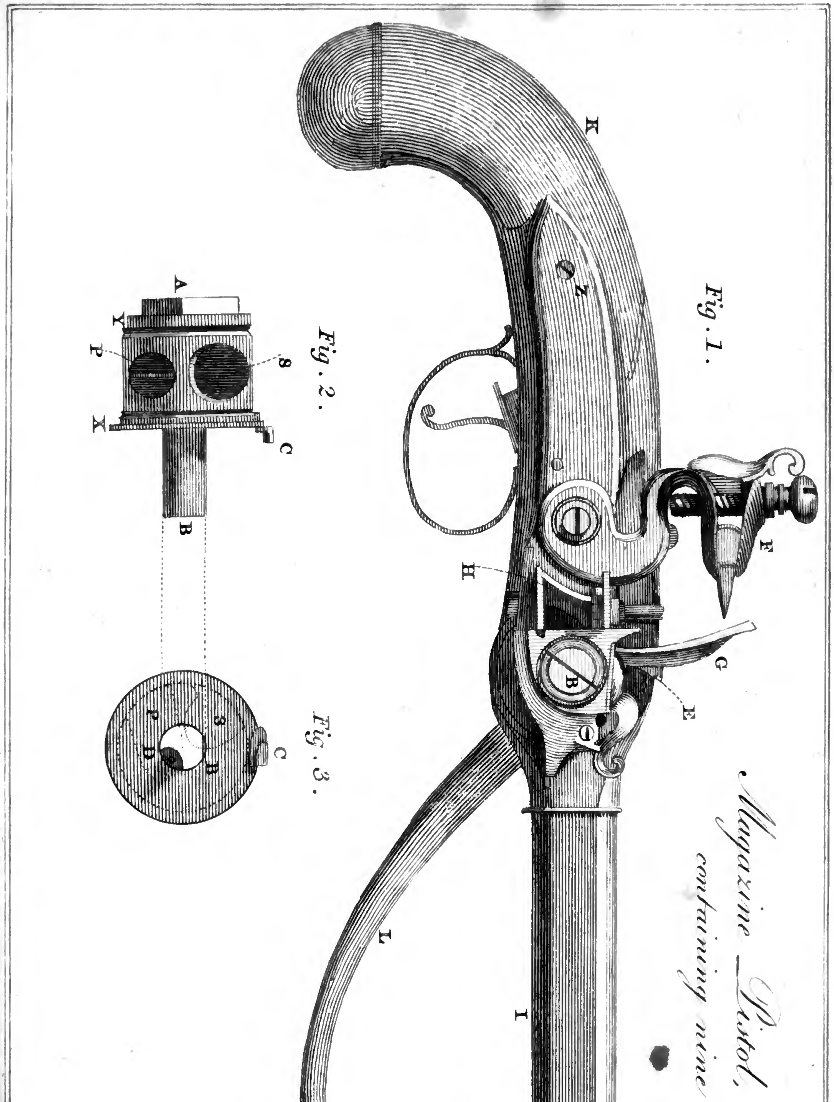

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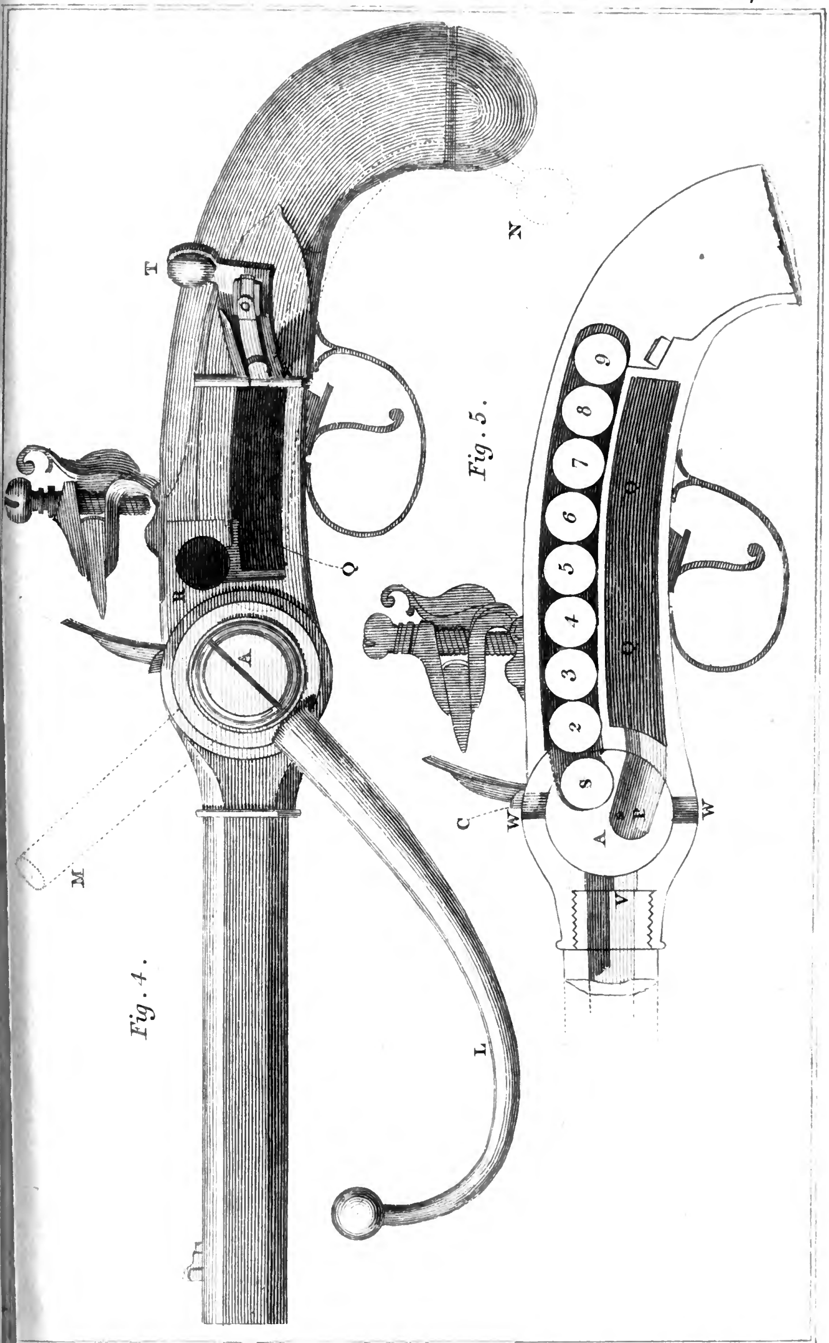

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